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THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

I. A NEW DETERMINATION OF THE MINIMUM VISIBLE AND ITS BEARING ON LOCALIZATION AND BINOCULAR DEPTH.

BY PROFESSOR GEORGE M. STRATTON.

The smallest lateral difference of place that is visible has until recently been given as about 50″–60″ angular measure. The method employed by Helmholtz and others[1] in reaching this result was the well-known one of bringing two parallel lines together until they finally are just distinguished as *two*—on the same general principle by which Weber determined the tactile space-threshold, by finding the distance between two compass-points that just cease to merge into *one*.

But by a different method it is now evident that a lateral difference[2] of place of about 7″ of arc can be directly perceived.

Instead of using lines or points side-by-side, the experiments which give this result were made with lines end-to-end, so arranged that the upper of two perpendiculars could be moved at will to the right or left while still remaining exactly parallel to the lower line, as shown in Fig. 1. The observer had simply to judge whether the upper line was continuous with the lower, or to which side it had been displaced. In the initial trials the lines were narrow slits of light surrounded by

[1] See Helmholtz: *Physiologische Optik*, 2d ed., pp. 256 et seq.

[2] In contrast with a difference in depth or a difference not of itself perceptible, but evident merely by reason of stereoscopic effect.

a black ground—each line not more than half a millimeter wide and some 10 cm. long, and observed in a half-darkened room from a distance of 18 meters. But the surprising fineness of discrimination made it impossible to work with accuracy from so short a distance; so that finally the observer was placed 120 meters away, and the lines were changed to narrow white strips, part of the time 10 mm., part of the time 8 mm., wide, each 50 cm. long, on a dull-black ground. Each line with its ground was mounted on the face of a plate of glass, the two glasses placed edge to edge in a frame; and, by sliding the upper plate upon the lower, lateral shifts were made by steps of 1 mm. At first the ordinary procedure of 'minimal changes' was adopted, but it soon became clear that suggestion was playing too large a rôle, and resort was had to a compromise between this method and that of 'right-and-wrong cases,' which might be called a method of serial groups, to be described more fully at another time. Taking as the threshold the point where 80%, or more, of correct judgments occurred, the following results were obtained:

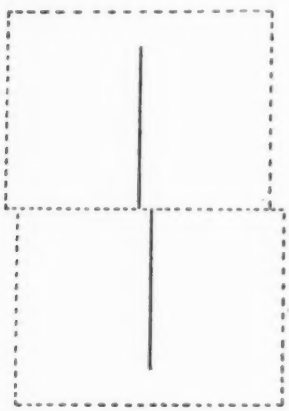

FIG. 1.

SUBJECT A. (187 judgments).

THRESHOLDS FOR DISPLACEMENTS TO THE

RIGHT.	LEFT.
2. mm.	5. mm.
4. "	6. "
3. "	5. "
Aver. 3. mm.	Aver. 5.3 mm.

General average 4.1 mm.

SUBJECT D. (277 judgments).

THRESHOLDS FOR DISPLACEMENTS TO THE

RIGHT	LEFT.
4. mm.	6. mm.
3. "	5. "
2. "	5. "
4. "	5. "
Aver. 3.2 "	Aver. 5.2 "

General average 4.2 mm.

Taking 1 mm. as equivalent, approximately, to 1.7 seconds of arc, where the radius is 120 m., we get a fraction over 7 seconds as the threshold of space-distinction under these conditions.

The experiments were begun with some doubt whether there was a direct perception of a *spatial* difference here; whether, for instance, some purely intensive change—some apparent dimming or strengthening of the impression from the adjacent ends of the lines where the displacement occurred—might not serve to suggest indirectly a spatial inequality. But anyone making the observation is soon convinced that what he sees is not of this character, but that the two lines seem to form a single line no longer straight. He seems to compare the position of one of the lines with an imaginary extension of the other, and to notice that the two, in this way, do not coincide. As a check, however, the observers throughout the experiments were kept in ignorance of the actual direction of the displacements, and the threshold was not considered as reached until the direction could be told by them; so that if their judgments had been based on anything other than the perceptible position of the lines, the fact of a break or shift in general might perhaps have been inferred, from some intensive difference in the light, but it is difficult to see how, from that alone, the subjects would have been able to tell correctly to which side the line had been moved. There seems, moreover, to be nothing inherently suspicious in the striking disparity between these results and those obtained by the older method. In the threshold obtained in Helmholtz's way these finer measurements are not reached simply because the diffusion of the stimulus from the two lines side-by-side makes a fairly uniform blur on the retina between them, and within the limits of the blur two separate objects can no longer be distinguished. But when the lines are arranged as in the present experiment, any such interference by mere diffusion is greatly lessened, and the localities can be clearly and correctly distinguished.

The result thus obtained is interesting in several ways. In the first place, it probably removes the grounds for inferring, as yet, that stereoscopic depth at its minimum is a *subconscious* result of the spatial conflict of the two images. My own ex-

periments with the pseudoscope[1] had shown that an angular difference of 24 seconds between the two impressions was sufficient to give a binocular relief. Still later, Bourdon,[2] experimenting with needles at short range, found that a difference amounting to but five seconds produces a perceptible depth-effect. As long as the conscious lateral threshold was counted as above 60 seconds, one would be tempted from these results to believe either that stereoscopic depth was not due primarily to a lateral space-discrimination of the two images, or else that the plastic effect must depend on a subconscious action, since the disparities in the image were smaller than could be consciously noted. The present reduction of the conscious lateral threshold to about 7 seconds leaves but a small margin upon which to base such conclusions, particularly when one takes into account the wide contrasts in the (non-essential) conditions of the experiments. Bourdon would seem entirely justified, however, in concluding that binocular depth cannot be due to our detecting double images, if by double images we are to understand outlines that are distinguishable *side-by-side*. But the depth-effect may still be due to the presence of double images in the sense of outlines that are felt not to be coincident when positions are compared *end-to-end*. There certainly is a difficulty in that ordinary stereoscopic vision seems hardly to provide the conditions for comparing outlines in this way. It is barely possible, however, that the curious phenomenon of retinal rivalry may be useful just herein, that by the successive emerging and disappearance of parts of the outlines in the two projected fields of view something comparable to the conditions of the present experiment is brought about, and an exceedingly fine perception of lateral incongruity results.

But perhaps a more important bearing of the experiment is on the general problem of visual localization. It seems highly improbable that so minute a displacement is discerned by noting some muscular jog or unevenness in running our eyes up and down the line, when one recalls that the fovea itself is some

[1] A Mirror Pseudoscope and the Limit of Visible Depth, PSYCHOLOGICAL REVIEW, Vol. V., p. 632.

[2] L'acuité stéréoscopique, *Revue Philosophique*, January, 1900.

300 times broader than the retinal image of the space-inequality here perceptible. In view of this relatively wide expanse of the fovea, it seems highly questionable whether the eye, in running up and down such a pair of lines would regularly take one course when the lines exactly met and a perceptibly different course when one line was displaced 7″. The eye naturally moves by twitches and jerks, even when following a straight line; the breadth of the fovea is such as to permit considerable roving without 'losing' the line. So that so small a dislocation in the objective line would probably be no incentive to an exactly corresponding change in the movement of the eye. And even supposing that a dislocation of 7″ in one of the lines did regularly tend to draw the eye by so much out of its course; how should we be able accurately to interpret so slight a variation of muscular action, as clearly due to an objective spatial inequality, when much greater movements—likewise involuntary, are constantly occurring without our interpreting them as due to a spatial variation in the object we are observing?

If, on the other hand, we pass from the muscular apparatus and look to the minute elements in the retina to explain such discriminations, even these seem much too gross to account for the marvellous fineness of our judgment. Rows of cones in the mosaic of the fovea lie apart a distance corresponding to at least an angular measurement of 30″. To explain our power to detect a spatial difference one-fourth of this, it will be necessary to assume either that the rods and cones are not the ultimate spatial elements in the retina, or else that the *minimum visible* may be considerably less than the distance between the center of the adjacent sensory elements.

And, after all, it is not difficult to see how this latter might well be the case. We must, however, first give up the notion that the light, even when it falls upon a single cone, or row of cones, affects only those elements upon which it directly falls. In every case the stimulation is probably diffused in all directions; a responsive wave runs through the neighboring elements, as is evidenced, for instance, by the fact of simultaneous contrast. Now if C and C' be adjacent elements, and b the boundary between them, then it seems not improbable that if a ray

fall somewhere within the limits of C its effect upon C' will be different according as its point of incidence is nearer or farther from b. It would probably excite C' more intensely the nearer it fell to the limits of this cone; and, on the other hand, its effect upon C'', a neighbor on the opposite side of C, would correspondingly decline. In such an event there would be a difference of nervous result with every change, however small, of the point of incidence, instead of a change occurring only in case the stimulus passed to an entirely different cone. There would then be a difference of intensity of the diffusive effect upon neighboring elements, a different degree of whatever kind of reaction may be characteristic of each, and consequently a change of 'local sign' even when the difference of place of the excitation is considerably less than the diameter of a single element. The local signs in the retina alone, quite apart from the muscular mechanism, would thus form an absolutely continuous series, and would furnish the data for any degree of spatial discrimination we may discover. The retinal sign of position is thus conceived, not often the analogy of our electric signal boxes, when one unalterable mark is given (a falling numeral, for example) whenever the same terminal is affected; but we should have to symbolize it rather by some more complicated contrivance: where nothing less than several neighboring numbers dropped on each occasion, but each of these appeared with differing clearness according as its particular terminal was near or far from the immediate origin of the disturbance. The relation of the various intensities could in this case be a sign both of direction and of distance; and the exact seat of the exciting cause be determined with a degree of accuracy depending on the fineness of discrimination for intensive differences and for catching their interrelation, rather than upon the number and distance apart of the several terminals; in other words, the threshold would not depend on the purely anatomical meas-

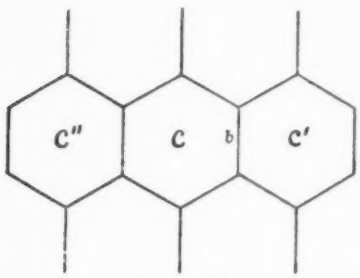

FIG. 2.

urements, if we may drop the figure and return to the retinal fact.

The present experiment, then, tempts one to believe that the local signs are of this exceedingly complicated character. The mental process of localization, or of space-distinction, cannot be justly described (it would seem) as an association merely between a particular quality of sensation and a particular place. Nor is it entirely sufficient to amend this and say that the mind must also take account of the various intensities of the quality which is spatially significant. The complication seems to go still a degree higher, so that the *interrelation* of numberless intensities of different retinal sensations would seem to be the intricate process involved in even the simplest visual perception of space. When we bear in mind that the fully organized perception without doubt includes also extra-retinal data, it is evident how complex an activity our spatial consciousness is.

II. The Effect of Imperceptible Shadows on the Judgment of Distance.

BY KNIGHT DUNLAP.

The conscious effect of stimulation of such low intensity as to be imperceptible presents an attractive and almost unexploited field for experimental work. The experiments of Jastrow and Pierce on small differences of sensation,[1] and some previous work of my own, suggested the possibility of obtaining important results from an experiment planned to show directly the effects of the presence or absence of an imperceptible stimulation.

The Müller-Lyer figure was selected as the foundation for such an experiment. If we have the segments of the principal line in the illusion-figure distinctly marked, but the angular lines of an intensity just below the threshold of perception, we have the simple materials for determining whether or not these imperceptible lines will produce in any degree the ordinary illusion-effect. If such an effect is produced, then we have

[1] Memoirs of the National Academy of Sciences, Vol. III., p. 76.

evidence for the belief that under certain conditions things of which we are not, and can not become, conscious have their immediate effects upon consciousness.

The apparatus used in the first investigations under these conditions was very simple, and not altogether satisfactory in its operation, but as the experiments were of the nature of a preliminary survey of the ground, it was not deemed advisable to make a complicated arrangement of apparatus.

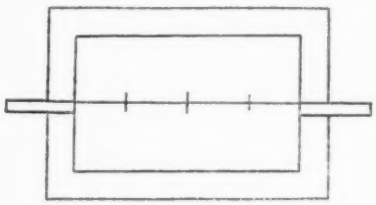

FIG. 1.

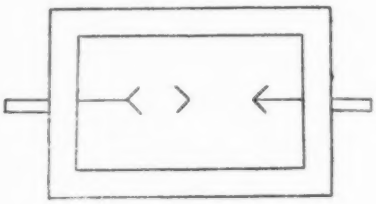

FIG. 2.

The screen upon which the illusion-figure was to be shown was a sheet of white bristol-board, fifty-eight centimeters by seventy-two centimeters, fastened upon a frame. A black line one millimeter in width (see Fig. 1) was drawn across the middle of the screen horizontally, and divided in the center by a perpendicular line three-fourths of a millimeter in width, and extending eight millimeters on each side. On the sides of the frame holding the screen, and on a level with the horizontal line were fixed guides, in which slid small steel rods passing along the line and hidden against it. Small strips of black paper two millimeters wide and ten millimeters long were fastened to the inner ends of these rods in positions parallel to the central vertical line and extending equally above and below the horizontal line, and these with the central vertical line marked off two segments of the horizontal line. The lengths of these segments could be varied at will by sliding the rods along the horizontal line.

Fastened to each of these rods carrying strips, and parallel to them, was another exactly similar rod passing behind the screen and bearing on its inner extremity an angle of ninety degrees, cut from black bristol-board, with legs five millimeters wide and

forty-five millimeters long. These angles, together with a similar one fastened in the center, showed upon the front of the screen as shadows when light was transmitted through from the back, and transformed the segments of the line into the typical Müller-Lyer figure. The arrangement of this part of the apparatus may be understoood from Figs. 1 and 2, which show respectively the front and back of the screen. It will be observed that by interchanging the pairs of rods and reversing the central angle the direction of the illusion could be reversed.

The screen was illuminated from the front by two hooded incandescent lights placed one at each side so as not to obstruct the view, and at equal distances from the screen. Behind the screen was a single hooded incandescent light, so shielded with tissue paper as to diffuse the light as evenly as possible over the back of the screen. The intensity of this light, and therefore the intensity of the shadows on the front of the screen, was controlled by means of a rheostat, the front lights remaining unchanged. As the current for the three lights was taken from the same circuit, variation in the potential affected them all in approximately the same ratio, and hence the relative intensity of the light coming through the screen to the light falling upon the face of it, as determined by the adjustment of the rheostat, might be supposed to remain fairly constant.

The method of operation was very simple. One of two figures, a double square and a circle, was placed against the back of the screen to test the intensity of the light, and the intensity of the rear light was reduced until the subject was unable to detect the shadows caused by the figure. Upon reaching a point at which it was certain that the subject could not tell whether the figure was circular or square, it was removed and the angles of the Müller-Lyer figure placed in position. It was determined by the toss of a coin whether the angles should be set in the 'short' or 'long' position,[1] and the left cross-line was fixed at twenty-five centimeters from the center. The right

[1] The direction of the angles which tends to shorten the left or standard segment of the line, thus <—>——<, will, throughout, be spoken of as 'illusion short.' The opposite direction of the angles, which tends to lengthen the left segment, will be spoken of as 'illusion long.'

cross-line was started from a point sufficiently greater or less than twenty-five centimeters from the center to be distinctly perceived as farther or nearer than the left one, and moved at regularly timed intervals[1] by steps of one millimeter towards and past the equality point, the judgment of the subject as to the length of the right segment of the line as compared with the left segment being recorded at every step in the series. Whether the cross-line should move inward from a point beyond the equality point, or outward from a point inside, was determined by lot, correction being made toward the close, however, so as to have on the whole as many series of one kind as of the other in order to offset the effect of mere direction of motion. It is evident from the details given above that the angle behind the right cross-line moved with it, so that the relations of the Müller-Lyer figure were constantly preserved.

Series of this kind alone would of course not be sufficient to determine whether or not the illusion figure is effective. As will be seen later, there is a tendency to judge the equal segments of a line as different even apart from any influence of the angles of the Müller-Lyer figure. Hence it was necessary for purposes of comparison with these to take also series in which there could be no possible illusion, since the light behind the screen was entirely cut off. If there should be any effect produced by the shadows under the conditions previously stated, a comparison of the series with those taken when the shadows of the angles were present but imperceptible might show it.

The series were consequently taken in pairs or sets, each pair being composed of one series with the shadows, and one without, taken in immediate succession, in the same direction, and from the same point. The order in which the two were taken was determined for each pair by lot, as was also the direction of the illusion as mentioned above. The subject being ignorant of the results of the lots, there was a double check upon the possibility of any influence arising from his knowledge of how the illusion might be expected to affect his judgments. Any general difference between the two classes of series could

[1] The interval was nine seconds in length from the completion of a judgment until the command to look again at the screen.

therefore only be due to the effect of the angles behind the screen.

In general, each series proceeding outward resulted in first a number of judgments of 'shorter,' then one or more of 'equal' or 'doubtful,' and finally, a number of 'longer.' In the series proceeding inward the order was reversed. The middle point of the region of doubt and equality was taken as the mean equality point of each series, and this was compared with the mean equality point of the other member of the pair. The region of doubt and equality was determined by fixed rules, and, in order to secure absolute impartiality the point was determined without the experimenter himself knowing to which class the series belonged. For this purpose a number of the records were allowed to accumulate, were shuffled, and their distinctive marks concealed until their mean equality points were determined and recorded.

Three subjects were employed, and the results of the work with them are summed up in Table I. A set was counted 'for' or 'against' the illusion according as the difference between the mean equality points of the two series composing it was or was not in the direction which would correspond to the possible effect of the illusion-figure.

TABLE I.

Subject.	Total Pairs.	For Illusion.	Against.	Neutral.
A.	23	14	8	1
R.	11	9	2	0
S.	13	8	5	0

We see from this table that 60 per cent. of the sets for subject A. fall on the side of the illusion, 81.8 per cent. for R., and 61.5 per cent. for S. The figures are rather meager, but as far as they go are strongly suggestive. As the experiments were designed only as preparatory to the investigation proper, to point to possible results and expose the difficulties in the way, the results were counted sufficient, and preparations were made for more careful experiments along the same line.

The apparatus used in the later work differed materially from that which was described above in the account of the pre-

liminary investigation. It was desired that there should be some means of measuring the relative intensities of the shadows, and as this was practically impossible when they were cast by light transmitted through the screen, it was decided to try the effect of casting the shadows directly upon the front of the screen. This of course necessitated the removal of the angles used in casting the shadows to some distance from the screen, in order that they might not be in the subject's line of vision, and this in turn demanded the use of light radiating from a very small area, that the shadows might be sharply defined. The best form of illumination available for this purpose was the electric arc, and as it is not possible to maintain the intensities of two arc lights at anything like a constant ratio, it was necessary to cast the shadows from the same source of light which furnished the general illumination of the screen. This was effected by the aid of mirrors, which diverted in opposite directions the light coming from the lamp, and combined it again upon the screen. An automatic adjustment lamp was first tried;

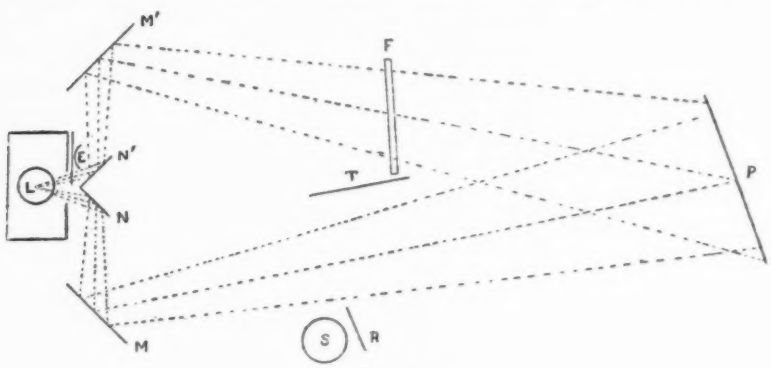

FIG. 3.

but as it proved too unsteady, a hand adjustment lamp, with carbons at right angles to each other, was constructed in the laboratory and found more satisfactory. The arrangement of the lamp with the other parts of the apparatus is shown by the diagram in Fig. 3. The lamp was placed in a box having a blackened interior and suitable apertures for the emission of light from the front, and for ventilation. The light reflected by mirrors *N* and *M* fell upon the screen *P* without interven-

tion, but that reflected by N' and M' was intercepted by the episkotister E, by which any desired proportion of the light was cut out. This light passed through the frame F, in which were suspended on silk fibers angles measuring sixty degrees (these being more favorable to the illusion effect than those of ninety degrees used in the preliminary work), and cast their shadows on the screen P. This screen was the one used in the preliminary work, with the exception of the angles at the back, which were superfluous in the present arrangement, and were therefore removed. It was placed in a position perpendicular to the line of vision of the subject at S, who was seated at one side, out of the path of the light, and hence the distribution of the light on the face of the screen was slightly unsymmetrical. This was unavoidable, however, on account of the dimensions of the room which was selected as best adapted to the purposes of the experiment. The walls, floor and ceiling of the room were black, preventing any great reflection of light back to the screen.

Some difficulty was experienced with the mirrors, and those of carefully selected plate-glass used in the experiment were not thoroughly satisfactory. Better results could probably have been obtained by the use of lenses and totally reflecting prisms.

The fibers by which the angle casting the right-hand shadow was suspended were attached to slides working in the frame F, so that the angle could be moved along as the right-hand cross-line was moved on the screen, thus keeping the vertex of the shadow approximately on the cross-line on the screen P. The principal remaining pieces of apparatus were screens, one at R to prevent the subject seeing the figure at P except at the proper time; and one at T to conceal the angles in the frame F at all times. The distance from the subject to the figure was about two and a quarter meters.

It will be observed that since the two pencils of light were taken from practically the same side of the glowing carbon, the effective area of which was only a few millimeters in extent, variations in the intensity of the light affected them both approximately equally as regards their initial intensities, and hence did not change the relative intensities as established by the episkotister. Since therefore the intensity of the light did

not vary greatly, the perceptibility of the shadows should have remained constant according to the general statement of Weber's law.

What has been said concerning methods in the preliminary work will, with some important exceptions, apply to the main work also. The proper intensity of the shadows was determined for each subject by careful tests, and these were frequently repeated during the course of the experiment to insure the correctness of the intensity adopted. It was found that subjects A., R. and W. could not detect any shadows when the total angular opening in the episkotister was seven degrees. For the sake of safety six degrees was used in the actual work with these subjects. S. at one time seemed to perceive the shadows with an aperture of seven degrees, and therefore five degrees was fixed upon as entirely safe for him. These proportions are rather large as compared with the figures usually given for the difference-threshold of light, but it should be remembered that the shadows were not perfectly distinct, on account of the effect of the fringe of luminosity surrounding the effective portion of the positive carbon, and also on account of multiple reflection in the mirrors.

As it took some time to change the direction of the angles in the frame, they were placed at the beginning of each experiment-period in the direction determined by lot, and continued in the same direction during the whole period (three-quarters of an hour), unless (as happened some few times) the subject accidentally became aware of the direction of the angles. In such a case a coin was tossed to determine whether or not the angles should be reversed, the subject being ignorant of the outcome of the lot, and so not knowing whether the shadows were continued in the position in which he saw them, or were reversed, or were entirely removed and a shadowless series commenced. Towards the end of the work the angles were arbitrarily placed so as to have an equal number of series for each of the two directions of the illusion. To economize time, the angle which cast the right shadow was not moved each time the corresponding cross-line was moved, but only every fifth time, the width of the shadow allowing that amount of movement without complete disconnection.

No attempt was made to run series in both directions, as it was not indispensable for the comparison of the series, and there were enough complications without this additional factor. All the series were run outward from a point nearer to the center than the standard length of twenty-five centimeters.

When the shadows were removed, in the shadowless series, by cutting off the light normally reflected by mirrors M' and N', the episkotister was kept running, and the angle was moved in the frame at the usual times in these series as in the others, so that the subject had no clue as to whether or not the shadows were present in any series.

For fear lest the imperfections in the mirrors might produce irregular distribution of the light on the screen in such a way as to affect the results of the series, the two mirrors on one side were interchanged from time to time with those on the other side, and an equal number of series taken in each of the two positions. There were thus three conditions to be equalized, viz: (1) Position of the mirrors; (2) Direction of the illusion; (3) Order of series in the set, *i. e.*, whether the series with the shadows was given first, immediately succeeded by the series without the shadows, or *vice versa;* in the scheme which follows the first class are called 'shadows first' and the second 'shadows last.'

This classification broke the sets of series up into eight groups, which were kept equal in the long run (with one exception, to be mentioned later), although the sets into which the series fell were determined by lot from day to day as far as possible. These groups are given schematically in the following table:

1st position of mirrors.	Illusion long.	Shadows first.
		Shadows last.
	Illusion short.	Shadows first.
		Shadows last.
2d position of mirrors.	Illusion long.	Shadows first.
		Shadows last.
	Illusion short.	Shadows first.
		Shadows last.

On account of the complication of the apparatus and the difficulties of its operation, the progress of the experiment was necessarily slow. No attempt was made to have the subject judge at regularly timed intervals. An endeavor was made, however, to give all series with the same approximate rapidity, and the rate of ten minutes to a set was pretty constantly maintained. This allowed the taking of three sets or pairs in a period of forty-five minutes, if all worked smoothly, the rest of the time being consumed in the necessary adjustments of the apparatus. Very frequently, however, troublesome delays occurred, which reduced the number of sets to two or one.

Each subject was instructed to preserve a fixed method of bringing his eyes upon the line. One preferred to bring his eyes upward from the floor to the middle of the line after removing the swinging screen, while the others preferred to gaze at the swinging screen, in the direction of the center of the line, and then displace it. This regularity was insisted upon because it was found by actual trial that the direction in which the eyes were brought upon the screen influenced the proportional estimate of the segments of the line. Therefore a difference in this respect between the different series might introduce a difference in the results which would confuse the interpretation.

The subject was not however compelled to maintain his gaze fixed upon the central point, but moved his eyes over the line as he pleased in forming his judgment. In the extreme cases the disparity of the segments of the line was perceived by glancing at the center, but where the difference was very slight the subject gazed at one segment until he had its length well in mind and then transferred his regard to the other segment in order to make comparison.

It is probable, however, that the steady fixation of the segments was on the whole a bad practice, as it rendered possible the formation of an after-image of the first segment by which the second segment might be judged by mere superposition, which would be in a large measure destructive of the effect of the illusion, if there were any such effect. The difficulties of judging were however so great that it was not deemed advisable to place any additional restrictions on the subject.

The experiment would naturally have been much simplified if the shadowless series had been omitted, and the sets of two made to consist of one with the illusion short and one with it long. This would have reduced the number of groups to four, and would perhaps have nearly doubled the difference between the series on each set, but it was deemed inadvisable for several reasons. First, the changing of the direction of the angles for each series would have consumed so much time that the gain by omitting the shadowless series would have been largely offset. Second, the data for the judgments of the segments of the line with the influence of the illusion excluded are important in themselves. And third, it was considered possible that under the conditions, the illusion, if effective at all, might be more effective in one position or direction than in the other; a state of affairs of which the proposed method would offer no evidence, should it actually exist. The results as set forth later justify the procedure actually adopted, although additional experiments ought to be made in the proposed manner to supplement the results of these.

One more point as to the methods, and then we may pass on to the next division of the subject. The series were always commenced far enough inside of the actual equality-point to give a distinct impression of shortness to the right segment of the line. They were not however commenced uniformly at the same point, but were varied irregularly in this respect from set to set, and the subject was given to understand that they were irregular, lest the distribution of his judgments in one series should influence him in the next. Both of the two series of each set were of course commenced at the same point, so that any possible effect of the length of the series, or the strength of the contrast with which the series began, might affect them both alike; but this was not mentioned to the subject, who consequently never knew whether a given series began at the same point as the preceding. It is interesting to note that the general region of commencing the series was necessarily varied from day to day and even from set to set on account of the varying relative estimation of the segments of the line. Thus a point which one day was well within the range of judgments

of 'short' might be the next day within the range of judgments of 'equal.' This will be further exemplified later in the discussion.

Four subjects were employed, three of whom, S., R. and A., had taken part in the preliminary work. In the cases of S. and R. eighty series (forty sets) were taken from each, evenly distributed according to the scheme of the eight classes of sets already specified; but in the case of A. only sixty-four series (thirty-two sets) evenly distributed, and in the case of the other subject, W., seventy-two series (thirty-six sets, twenty under the class 'long' and sixteen under the class 'short') were obtained.

The most obvious method of comparison of the series was by sets, taken in the same manner as that in which the series in the preliminary work were compared. The same precautions for impartiality in computation were observed here also. The results of this comparison as given in Table II. are far more striking than the results for the preliminary work.

TABLE II.

Subject.	Total Sets.	For.	Against.	Neutral.
S.	40	23	15	2
W.	36	19	11	6
R.	40	25	9	6
A.	32	22	7	3

Expressed in percentages this table becomes Table III., as follows:

TABLE III.

Subject.	% for.	% against.	% neutral.
S.	57.5	37.5	5.0
W.	52.8	30.5	16.7
R.	62.5	22.5	15.0
A.	68.8	21.9	9.3

This shows a preponderance for the illusion; twenty per cent. for S., over thirty-two per cent. for W., forty per cent. for R. and over forty-six per cent. for A. Or, if we leave out of account the neutral cases and give directly the ratio of favorable to unfavorable cases, we find the following values: S., 1.5.

W., 1.7; R., 2.8, and A., 3.1, which are certainly too large and agree too well to be set down as the result of mere chance.

A second comparison naturally suggested would be between the average of the mean equality-points for the shadowless series and the averages for the series with the illusion-figure in both positions. An examination of the series, however, showed that the average for all of the shadowless series could not be fairly compared with the averages for each of the other two classes, since the series for the two latter occurred in groups irregularly distributed throughout the time the experiment was continued, and the general position of the mean equality-point varied greatly during the progress of the work.

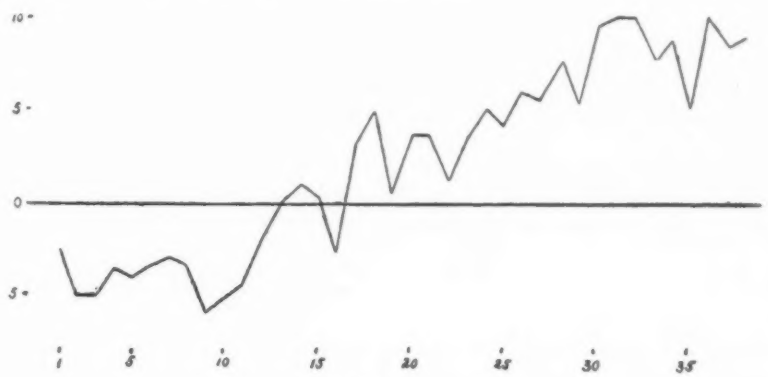

FIG. 4.

This is illustrated by Fig. 4, which gives the position of the mean apparent equality-points for each of the shadowless series of experiments with subject W. In this diagram the values of the abscissa represent the order of the series in the course of the experiment, while the ordinates give the difference in millimeters between the standard line and that length of the variable which seemed equal to it. Positive ordinates, therefore, show that the apparently equal variable was actually longer than the standard, while negative ordinates indicate that it was shorter.

All of the graphs thus obtained were very irregular, and those for subjects W., R. and A. show a decided upward tendency from first to last, indicating that while early in the experiment the right segment was proportionately overestimated, later it

was underestimated. Subject S., with a single exception, over estimated the right segment throughout the experiment, rather more toward the last than the first.

The graphs must not be understood as analogous to the filling in of the points of a probable curve between known points, as in this case the points represent discrete series and there are no possible points which might be computed between them. Only the known points are significant therefore, the lines joining them having been added to distinguish the trend of the series and make it more easily comprehended by the eye.

The ordinates for the series with shadows would correspond pretty closely with the ordinates for the shadowless series with which they belong; but, as previously noted, the two classes of series with shadows were not distributed regularly over the interval covered by the shadowless series. In the case of one of the subjects, for example, the fall of the lots was such that most of the series with illusion 'long' occurred at the upper position of the graph for the shadowless series, and those with illusion 'short' at the lower positions.

It would, therefore, be manifestly incorrect to compare series corresponding to the upper part of the graph with series corresponding to both upper and lower parts, as this would bring in a difference due merely to the progress of the experiment.

TABLE IV.

AVERAGE MEAN EQUALITY POINTS.

Subject.	Illusion Long.		Diff.	Illusion Short.		Diff.
	Shadowless.	Shadow.		Shadowless.	Shadow.	
S.	−6.42	−5.8	+.62	−5.57	−5.75	− .18
W.	+2.37	+2.77	+.40	+2.65	+1.75	− .10
R.	−3.57	−3.40	+.17	−2.40	−3.45	−1.05
A.	−0.37	+0.46	+.83	+1.06	+0.78	− .72

Hence in computing the averages, those for the shadowless series which were taken at the same time as the series with the illusion 'long' were kept separate from the averages for the shadowless series taken at the same time as the series in which the illusion was 'short.' In other words, the averages for the two classes of pairs, as distinguished by the direction of the

illusion, were taken independently. The results for this operation are given in Table IV.

The figures in the first, second, fourth and fifth columns of this table represent the distance in millimeters from the actual

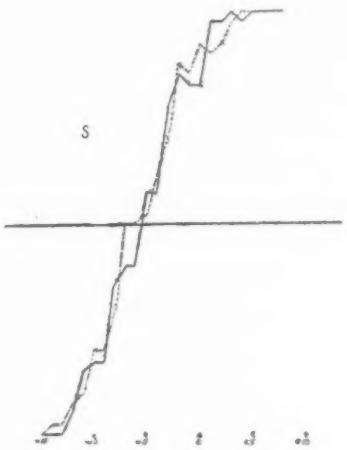

FIG. 5.

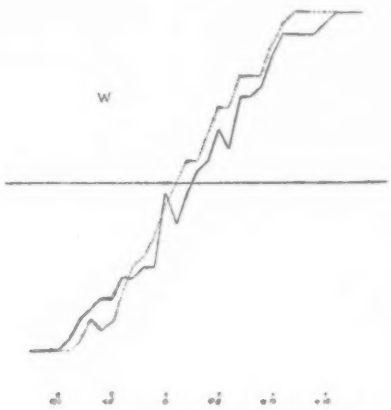

FIG. 6.

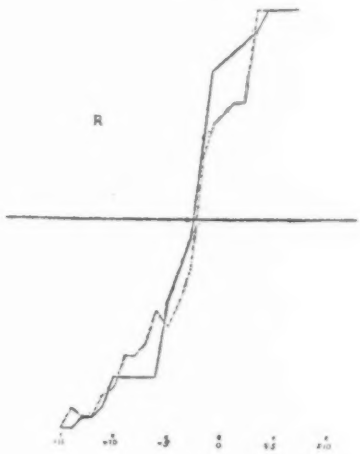

FIG. 7.

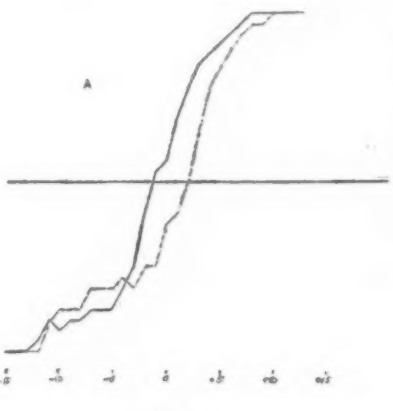

FIG. 8.

equality point to the average mean equality point for each group of series as indicated by the column headings. Negative distances are measured from the actual equality point towards the center of the line in the illusion-figure, and positive distances in

the opposite direction. The figures in the third and sixth columns give the differences of the averages, and hence directly the average change in length of the right segment when the imperceptible shadows were present.

It will be seen from Table IV. that in every case the average mean equality point for the series with shadows is farther out than that for the shadowless series when the illusion is 'long,' and just the reverse when the illusion is 'short'; which is exactly what might be expected if the illusion were really operative. This unanimity is certainly strongly in favor of the theory that the imperceptible shadows actually affect the judgment. The difference here, as in the other comparisons, is slight, but we should hardly expect to get more than a slight effect from the shadows under the circumstances.

This method of comparison by means of equality points necessarily leaves out of account certain scattering judgments which are bound to occur from time to time, and which are ignored by the fixed rules under which the mean equality point was determined in each series. We must, therefore, have some other method of comparison which shall take these into account, although their importance is not so great as is that of the more regularly occurring judgments. The sporadic judgment, contradicted by those which immediately precede and follow it, is of course largely due to a sudden subjective change in the observer, but its significance lies in the fact that the objective conditions might allow these subjective conditions to be more effective in one case than in another.

The method adopted for doing justice to these scattering judgments was the comparison of the total number of judgments of each kind (*i. e.*, 'longer,' 'shorter,' and 'equal' or 'doubtful') for each value of the variable lines on the screen, for each of the two classes of series with the illusion shadows, with the corresponding classes without the shadows. Thus, for example, the totals of the different kinds of judgments when the variable was 23.5 cm. (the standard being 25 cm.), when the illusion was 'long,' were compared with the totals for the same point in the corresponding shadowless series; and so for all of the other lengths of the variable which were used in the experiment.

Here as before, the precaution was taken to compare each of the two classes of series in which the shadows were present only with the shadowless series taken in the same sets.

There being three kinds of judgments, viz., 'longer,'

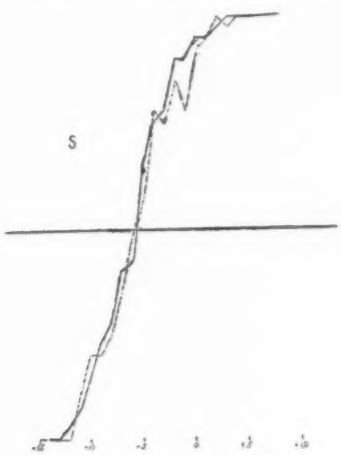

FIG. 9.

FIG. 10.

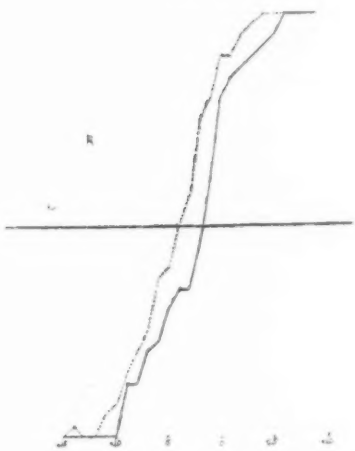

FIG. 11.

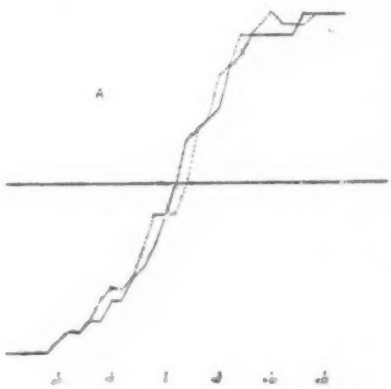

FIG. 12.

'shorter' and 'equal' or 'doubtful,' there were then for each class three totals for each point in the scale. As each of these totals is significant only in so far as account is taken of the distribution of the remaining judgments between the two remain-

ing totals, no direct comparison of the total judgments 'longer,' *e. g.*, in the shadowless series, with similar judgments in the opposite series could be made with advantage. Accordingly the judgments of 'short' were taken as negative, the judgments of 'long' as positive, and the judgments of 'equal' or 'doubtful' as zero. The algebraic sum of these judgments then, at a single point on the scale when the shadows were present, or, on the other hand, when they were absent, fairly represents the character of the total of the judgments at that point. As the series were always begun well within the region of apparent 'shortness' and ended well outside of the region of equality, it was assumed that it was possible to supply judgments at either end of those series that did not reach the limits of the extent of the scale taken for comparison, thus keeping equal the total number of judgments throughout the scale.

Figs. 5 to 12 give in a graphic form the results of this comparison. The abscissas represent the different lengths of the variable line expressed as distances in millimeters from the point where standard and variable were equal; negative values, of course, representing a variable shorter than the standard, and positive a longer. Hence the values here given, plus twenty-five centimeters, show the actual length of the variable. The ordinates represent the algebraic sum of positive, negative and zero judgments. The continuous line gives the value for shadowless series, the dotted line that for series with shadows—in Figs. 5 to 8 so placed as to give the illusion 'short'; in Figs. 9 to 12 'long.'

When treated in this way the results are less striking than in the case of the tables of general averages. If an effect of imperceptible shadows were to appear in the graphs, it would in Figs. 5 to 8 be evidenced by the fact that the ordinates of points on the dotted lines would incline more toward positive values than the corresponding ordinates of the continuous lines; and more toward negative values in Figs. 8 to 12. This would bring the dotted line to the left of the continuous line in the first group of figures, and to the right in the second group. In some of the actual curves, however, the dotted line shows no decided tendency either way (Figs. 5, 8 and 9). In Figs. 6, 10 and

11 the relation is faintly in accord with what we should expect if the shadows were effective; while in 7 and 12 the agreement is quite striking. If this accord were a mere matter of chance, several of the curves ought to be not simply neutral, but positively opposed to this course. None of the graphs, however, has this character. So that, on the whole, this third mode of treating the results also is favorable to the view that the shadows were operative in determining the judgment.

We may now sum up the results of our investigation. We have found three main points. First, when we compare the series set by set as they were taken, we find that for each subject the large majority of the sets compare as they should if the illusion were operative. Second, we find that the averages of the sets compare without exception as they should under the foregoing hypothesis, a result which does not necessarily follow from the foregoing result. Third, we find that the totals of the judgments at the various points on the scale are, to a remarkable extent, in conformity with the same hypothesis. These three methods of comparison cover every relation of the series which can fairly demand attention, and hence may be taken as exhaustive of the results of the present experiment.

A question of such importance as that with which the present discussion deals requires, however, the maximum of careful investigation before we dare call it settled. The results detailed above strongly suggest that the imperceptible illusion-figure is active in producing psychical results, but for the sake of conclusiveness additional experiments should be carried out so as to permit the comparison of series in which the shadows are in one position directly with other series in which the shadows are in the opposite position, using a modification of the method of right and wrong cases, consisting in plotting the error curve for the progressive intensities of the shadows by the method of Least Squares, and then comparing this with the empirical results obtained by the actual trial of the different intensities. This method of procedure is not absolutely necessary, but is advisable on account of the added satisfaction derived from approaching the same problem by diverse methods.

VISUAL PERCEPTION DURING EYE MOVEMENT.

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SECTION I. TWO SOURCES OF ERROR IN INVESTIGATING THE PROBLEM.

The problem of determining the character of visual perception during the movement of the eye from one point of fixation to another presents some exceptional difficulties to a psychological investigation.

During the visual examination of an object of any considerable size, say 5°, we are commonly conscious of a series of eye movements 'following the contour lines,' etc. On the other hand, the careful examination of a minute part of such an object certainly demands more than a continuous movement of the point of regard across it. We fixate it directly for an appreciable length of time.

If one endeavors to distinguish by mere introspection between the visual data acquired during the eye movements and those of the fixation pauses, an insurmountable difficulty will be discovered, viz: All self-observation of the eye movements and fixation pauses is utterly unreliable.

Attention has recently been called to this difficulty in a number of ways. It is now well known, for instance, that the eyes make a very regular series of full stops in reading; but even the trained psychologist is unable to give a reliable account of the points fixated at these stops, or even of their number, from mere self-observation.

A less experienced observer is quite unable to tell whether a given eye movement was broken by a stop or not; and even after more than five years' study of the eye movements, I have no confidence in my own judgment in the matter, if it is

unverified either by the direct observation of an assistant or by some form of registration.

Direct observation by an assistant is by far the simplest and least questionable control. It has been used throughout the following experiments. Only one precaution is worth mentioning; and that is so obvious from general principles that it needs no elaboration. The assistant must not attempt to follow any point of the moving eyeball in its excursions; but must rather fixate some point of the lid or eyelash, which is relatively at rest. In some cases it may even be necessary to construct an artificial fixation point, independent of the eye.

While this fundamental difficulty is so easily overcome by the simplest experimental conditions, it is obviously not impossible that, without these, it may have led even trained investigators into believing they saw during eye movement that which was really seen during an unnoticed fixation pause.

A second source of error in the investigation of our problem is the familiar difficulty of distinguishing the centrally conditioned elements of a percept from those which are peripherally conditioned. It is perhaps most widely known as the proof-reader's fallacy. A striking example is a phenomenon, well known to all who have worked with the tachistoscope. When a long word or phrase is grasped, even those parts seem to stand out clear and distinct, which a continuous exposure shows to have been so far out of the field of direct vision as to present a hopelessly indistinct blur.

We have no subjective criterion by which to distinguish the perceptive from the apperceptive elements in any given percept. Hence it follows that when we clearly recognize parts of a field of vision which a careful test shows to have been peripherally indistinct during all of the fixation pauses of the eye, the conclusion is almost inevitable that the clear perception was due to clearer vision during eye movement.

This second source of error is not as easily verified and controlled as the first; nevertheless it is the first thesis of this paper that the apparent clear perception of an object during eye movement is an *illusion*, due to these two fallacies of self-observation.

Section II. Clear Perception of a Complex Field of Vision During Eye Movement is Impossible.

The first attempt to determine how much of a complex field of vision could be apprehended during the uninterrupted movement of the eyes was made, so far as I am aware, by Professor Benno Erdmann and the author,[1] in an attempt to explain the meaning of those strangely rhythmic pauses of the eye in reading every page of printed matter.

It was then we chanced on the observation that when the head was held perfectly still we could never catch our own eyes moving in a mirror. One may watch one's eyes as closely as possible, even with the aid of a concave reflector, whether one looks from one eye to the other, or from some more distant object to one's own eyes, the eyes may be seen now in one position and now in another, but never in motion.

This seemed to me and still seems explicable in only one way.

The simple experiment is singularly precise. Since the phenomenon to be observed only occurs while the eye is moving, a pause in the movement, even if unnoticed, can do no harm. Even the apperceptive illusion has no chance, since it would tend, not to constructive imagination of a movement, but rather towards a greater clearness of the peripheral parts of the field of vision. There are two new sources of error, however, which demand experimental precautions. Slight, involuntary movements of the head produce conditions in which the eyes seem to move. They are best prevented by a Helmholtz head rest. Again, any abnormality in the movement of either eye, which causes it to move slower than the other, will make the end of its movement visible to the eye already at rest. This makes it advisable to confine the experiment to a single eye, shutting out the other by a screen.

The same lack of clear perception during eye movement may be discovered when the object is stationary, provided one takes the necessary precautions against the ever-threatening illusions.

If one fixates the beginning of a line on this page, for ex-

[1] Psychologische Untersuchungen über das Lesen, von Benno Erdmann und Raymond Dodge, page 69 et fol.

ample, the words in the middle of the line will be altogether illegible. The same will be true when the right end of the line is fixated, though the general character of the line is seen from both fixation points. If we look quickly from one end of the line to the other, it will seem as though we saw the letters and words during the movement, but an analysis of what was seen will show that we have not seen a particle more than we saw from one fixation point or the other.

This was the experiment by which Professor Erdmann and the author demonstrated the fact that the rhythmic pauses in reading are the moments of significant stimulation. Doubtless most who try it carefully will fall victims to the first difficulty. It will be clear that when the eye moves as rapidly as possible from one fixation point to the other nothing new is seen; but it will seem that, when the eye moves more slowly, the entire line is seen very distinctly. If the observer takes the precaution to have some one watch his eyes, as recommended, he will find that what in self-observation passes for a slow movement of the eyes is in reality broken by one or more clearly defined full stops. An assistant watching the observer's eyes will be able to predict with absolute correctness when the middle of the line was clearly seen, and when not, by the presence or absence of these full stops.

The rapid alternation of black and white stimulation by a line of letters is not the only condition of eye movement blindness. If a simple letter or figure is placed between two fixation points so as to be irrecognizable from both, no eye movement is found to make it clear, which does not show a full stop between them. Four or five lines, close enough together to prevent counting, may be substituted for the letter with the same result. This is more easily explicable when the lines are perpendicular to the line of movement, but the same result also obtains when they are parallel.

Any attempt to explain these phenomena by reference to the known laws of the action of a rapid succession of stimuli is met by the problem, which Professor Cattell emphasized in a recent significant article;[1] viz, why we get no characteristic lines

[1] On Relations of Time and Space in Vision, by Professor J. McKeen Cattell. PSYCHOLOGICAL REVIEW; vol. 4, 325 et fol.

of fusion. It is true that I believe I can detect such faint gray lines of fusion; for example, during long eye movements across a page of print; but the great majority of those I have tested discover nothing of the sort.

Section III. The Hypothesis of Anæsthesia During Eye Movement.

The possibility early occurred to me that the movement of the eye might condition a momentary visual anæsthesia. This seemed to be supported by the fading of after images during eye movement and similar phenomena. This hypothesis is, however, false. It is rendered improbable by the perception of long streaks of light seen when the retina moves across the image of a bright point of light on a dark ground. It is disproved by the following experiment:

A disk of black cardboard thirteen inches in diameter, in which a circle of one-eighth inch round holes, one-half inch apart, had been punched close to the periphery all around, was made to revolve at such a velocity that, while the light from the holes fused to a bright circle when the eye was at rest, when the eye moved in the direction of the disk's rotation from one fixation point, seen through the fused circle of light, to another one inch distant, three clear-cut round holes were seen much brighter than the band of light out of which they seemed to emerge. This was only possible when the velocity of the holes was sufficient to keep their images at exactly the same spot on the retina during the movement of the eye. The significant thing is that the individual round spots of light thus seen, were much more intense than the fused line of light seen while the eyes were at rest. Neither my assistant nor I was able to detect any difference in brightness between them and the background when altogether unobstructed.

In passing, it may be worth mentioning that the required result was reached when the holes had a velocity of one inch in 22σ, and the arc of eye movements was $4.7°$

The lack of suitable regulation for my disk prevents my following out the experiment; but it is certainly striking that this result is much closer in accord with my measurements of

the eyes' velocity by the Lamanski method, than with the measurements obtained by the eye cup and pointer method by Huey. The experiment seems well worth while carrying further by some one who has access to suitable apparatus.

Section IV. Perception of a Simple Field of Vision Exposed Only During Eye Movement.

In all the direct experiments thus far published, except the self observation in a mirror, the influence of the vague peripheral image of the object seen during the full stops at the first and second fixation points unquestionably interferes with any attempt to determine just what is seen during the eye movement. The tendency is towards a belief that the same amount is seen during the eye movement as at the more favorable fixation point.

Adequate experimental conditions for the investigation of the positive side of our problem demand a complete separation of the fields of vision during the movement and during the periods of fixation. An exposure apparatus is required, depending for its action on the movement of the eyes, so that objects will be exposed during movement and covered when the eye is at rest.

The conditions for such an arrangement are furnished by the movement of the eye itself. Since the axis of rotation is within the eyeball, in every eye movement the pupil travels through an arc equal to the arc of movement of the point of regard. If the arc of movement is large enough, the pupil may be made to travel the entire width of an open slit in a screen, through which objects behind the screen will be exposed as the eye is passing the slit, while they will be completely hidden by the screen at both termini of the movement. Taking 24 mm. as the diameter of the eyeball and 5 mm. as the ordinary maximum diameter of the pupil in daylight, it will be seen that the arc of movement which would carry the pupil entirely across a slit 5 mm. wide would be 72°. In practice, however, it is better to have the slit somewhat smaller and the arc of movement about 80°.

The necessary apparatus may be easily built up from material in every laboratory. As used in the first part of the fol-

lowing experiments it was essentially a simple perimeter, fixed in the horizontal plane, to which was attached a firm head rest and a special eye-piece. This latter was composed of two screens of black cardboard, set at an angle of about 30°, converging immediately before the eye, and so adjusted that when the eye looked at the center of the perimeter it looked between the screens through a slit 4 mm. wide, standing about 4 mm. from the cornea. The length of the exposed surfaces may be 15° or even more. The best results were obtained when they were not longer than 10°. All reflection from the black cardboard should be carefully shut out of the eye by secondary screens. For fixation points small bright steel rods were used. They effect a minimum retinal area and are very distinct. Whatever the fixation point used, each should be visible from the other.

The first experiments with this apparatus were altogether negative. White cardboard filling the whole ten degrees of the field of exposure was wholly unseen. The result was the same when colored surfaces were substituted for the white.

Fearing the unusually long movement might be an unfavorable condition, a modification of the apparatus was devised to shorten the arc of movement to 50°. The plan of this modification was to substitute the insensibility of the blind spot for the opacity of one of the screens of the eye-piece. It was effected by reducing the field of exposure to 3.5°, removing the screen *a*, and placing the primary fixation point in such a position for each observer that the field to be exposed fell completely within the blind spot.

For convenience of comparison the two pieces of apparatus were made entirely separate; the second form being built up on an ordinary perimeter.

This apparatus has an especial experimental value besides shortening the arc of movement; viz, the exposure is made during the first part of the movement instead of in the middle. It is, however, not without its disadvantages. For some eyes the primary fixation point must be 1°–2° higher than the field of exposure, if the latter is to fall comfortably within the blind spot. This renders the observations in so far questionable as

the field of clearest vision is not stimulated by the exposure. Moreover, the movement can be made to advantage with each eye in only one direction; since it is impossible to look from the second fixation point to the first with sufficient accuracy to insure the complete disappearance of the field of exposure within the blind spot, in a single uninterrupted movement.

The results of the first experiments with the new form of apparatus were also entirely negative. Five different colors and white were exposed in the field five times each for both my assistant and myself, with the result that, when the eye movement was unbroken, the observer was unable to tell what had been exposed or even that anything at all had broken the black of the perimeter. My own primary fixation point was directly in line with the upper edge of the field of exposure. My assistant's was 1°30′ above.

Subsequent experiments under exactly the same objective conditions have given quite different results. But I believe these first experiments are important as showing the difficulty of perceiving the simplest possible change in the field of vision, during eye movement, without special training. It is certainly suggestive of a central explanation for the absence of bands of fusion under ordinary conditions.

These failures suggested an increase of the illumination of the field of exposure. A ground glass plate, illuminated from behind by a bright, sunlit landscape, was substituted for the white cardboard in the first form of apparatus. Under these conditions a long band of light was immediately evident at each movement of the eye. This band uniformly appeared longer than 10° even when the length was known. There were no sharp boundary lines; particularly was this true in the direction of movement. This is probably due to a peculiarity of the apparatus, which is of great service in experiments on fusion. Each screen bounding the open slit is really an opaque body, intervening between the eye and the object, and exposing first one part and then the rest as the edge of the pupil moves across its edge. If the slit is near the eye, however, this function is entirely overshadowed by another, viz: that of a diaphragm, increasing and decreasing the brightness of the image

on the retina. Thus, when the pupil moves into the pencil of rays which has passed the slit, a point may be found at which a very faint image of part of the 10° field of exposure can be seen before the other part comes into view. This first faint stimulation, as well as the corresponding stimulation as the pupil passes out of the pencil of light, affects a portion of the retina not further stimulated, giving rise to the very faint ends of the band of light as it passes off into black. In experiments with a complex color field these ends are too faint and too far from the field of clear vision to cause any inconvenience.

Except for the indefinite edges, the band of light appeared homogeneous, but very much less intense than the ground glass during direct fixation.

If colored gelatin plates were placed in front of the ground glass, the colors, though dull, were always distinguished from gray and from the other primary colors.

Under the influence of these experiments, a return to the previous conditions showed a decided difference in ability to see the bands of light. At best, however, they had a vague cloud-like appearance, and the colors were mere tintings of this cloud.

Section V. Perception of a Complex Field of Vision Exposed only During Eye Movement.

The experiments have thus far dealt only with the simplest possible fields of light or color; but the qualitative modification of these fields by the movement of the eye clearly indicates the influence of their background. The problem of this influence is of the highest theoretical importance. Professor Cattell holds that the differences in the field of vision do not fuse during eye movement; but that, on the contrary, its various elements may be distinguished with extraordinary ease.

It is certain that we do not ordinarily find in consciousness any sign of a fused field of vision during eye movement. The long failure of physiological optics to correct the illusion of clear vision during eye movement is undoubtedly due in large measure to this lack of a fusion consciousness. In spite of it, however, it is now possible to show experimentally that when

a complex field of vision is perceived during eye movement it is seen fused.

A part of a page of print exposed in place of the white cardboard in the original apparatus gave a perfect though shadowy series of gray bands on a lighter gray background, in which individual letters or words were absolutely irrecognizable. The gray bands seemed homogeneous, without sharp boundary lines and of uncertain length.

Two white surfaces separated by a vertical black stripe, 2° 30′ in width, gave a homogeneous gray band, again without sharp boundaries and of uncertain length. There was no trace of an independent black stripe.

The last form of apparatus described was by far the most satisfactory arrangement for the study of color fusions. In front of the ground glass plate, illuminated as before by a bright landscape, was brought a black screen, perforated by four vertical slits which could be filled with colored films at pleasure. Each slit was 1° in width and four times as long as it was wide. They were separated from each other by equally large sections of the opaque screen. In front of this color screen was placed a movable screen, with teeth rising to the middle of the slits in the first screen, so arranged as to cover at will the lower half of either set of alternate slits. Since the same color always filled the alternate slits of the color screen this gave a valuable basis for the comparison of the dull band of apparent fusion of colors, with a juxtaposed band, first of one color and then of the other, as the sliding screen was adjusted.

The resulting fusions were thoroughly normal.

Colorless bands resulted from filling the alternate slits with complementary colors. Red and yellow gave an orange band; red and blue a purple one.

Although the difference between the upper and lower half of the field, *i. e.*, between the mixed and the simple colors, was always clear, the bands had throughout the characteristics previously mentioned. They were elongated, dull in color, and had uncertain boundaries. In no case of unbroken eye movement were the contents of the different slits distinguishable.

Section VI. The General Failure to See a Fused Field of Vision During Ordinary Eye Movement.

The question naturally arises why we fail to notice the fusion which the experiments clearly show must occur with almost every movement of the eye from one point of regard to another. Their persistent absence is, I am convinced, centrally as well as peripherally conditioned. It is my opinion that the difference between the results of the first and second series of experiments, with the first apparatus, under the same objective conditions, was due chiefly to a difference of apperceptive predisposition and attention; clearly indicating a most important factor in any explanation of the non-perception of the faint stimuli during ordinary eye movement. If our attention were habitually attracted by them, it must seriously disturb our clear vision at each new fixation. We should, consequently, rather expect them, like the entoptic phenomena, to be systematically ignored and even to require experimental conditions to demonstrate their existence.

In respect to the peripheral conditions, we must remember that it is only in complex fields of vision that the problem has any meaning. If the field of vision is simple or homogeneous in the planes perpendicular to the axis of rotation of the eye, stimulation during eye movement could not be differentiated in any way from that during the fixation pauses.

In any complex field of vision, the successive stimulation of each point of the retina by its various parts must tend to even up the total stimulation of those parts affected, and to break down the sharper contrasts of the fixation pauses. This is essentially a process of equalizing retinal excitation and of preparing for a new fixation.

Moreover, it must be remembered that the duration of the eye movements is very short, for movements of 10° certainly not more than 60 σ, and possibly less than half that. It will be seen at once that the equalizing stimulation during the movement of the eyes scarcely covers the duration of the most pronounced after-effect of the intense stimulation at the preceding fixation pause; while the interference of the intenser stimulation of the succeeding fixation would make the faint stimulation

hardly perceptible, even if it had no after-effect of the previous fixation, and no central inhibition to overcome in establishing itself.

There is strong evidence, with the details of which this article may not be burdened, that the equalization of the stimulation of each reading pause by the fusion during eye movement to the next fixation point is a very essential condition in the elimination of fatigue in reading, and as well in the prevention of error.

MENTAL FATIGUE. I.

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This paper is intended to be the first of a series of reports of a study of the nature of mental fatigue, some of the conditions affecting it, and especially its phenomena in school life. I hope also to discuss the methods and results of the many workers in this field in the light of some of the conclusions apparently justified by this study.

It seemed wise not to undertake any statistical investigation of fatigue in school children until the fact of mental fatigue had been carefully studied at some length in a few cases. The present section deals, therefore, entirely with the results attained by a study of subjects whose mental history during the experiments the investigator knew, whose method of work he could control and whose phenomena he could therefore interpret with some degree of assurance. It will also be wise to reserve any references to the work of others (of whom Professor Kraepelin is the most important) until these new data are presented.

The present paper may best be introduced by a quotation from a preliminary notice printed in *Science*. "Mental fatigue may mean either the fact of incompetency to do certain mental work, or a feeling of incompetency which parallels the fact, or the feeling or feelings denoted by our common expressions 'mentally tired,' 'mentally exhausted.' Among the conclusions to which the experiments have led are the following: First, that the fact of incompetency is not what it has been supposed to be; second, that there is no pure feeling of incompetency which parallels it and is its sign, that consequently the mental states ordinarily designated by the phrases mentioned are not states made up of such a feeling of incompetency but are very complex affairs, and third, that these mental states are in no sense parallels or measures of the decrease in ability to do mental work.

We have been accustomed to think of mental work in terms of mechanics. The mind has been supposed to lose its power to work as a rubber ball loses its power to bounce. As the ball rebounds to a lesser and lesser height, so the mind has been supposed to think with less and less vigor. We have talked as if sleep charged the mind with mental energy as a current might charge a storage battery with electricity, and that then the mind had this stock to spend. As it spent it, it could exert less and less energy in its thinking. According to such a view, after enough work has been done to get up a momentum, the amount of work the mind could do should steadily decrease in proportion of some sort to the amount it had done since its last rest. And the practical corollary (of tremendous importance) to such a theory would be that, again barring the preliminary start which acts as a stimulant, a man was always mentally exhausting himself, that in every day's mental work we normally approached mental debility, that mental work brought the curse of mental incompetency as its constant shadow, large when it was large, small when it was small. One would then be prevented from doing much mental work when one's mind needed rest simply by being unable to do it.

Another theory would be that mental work had no such constant warning attendant, but that certain extraneous results sufficed in the realm of natural mental hygiene to prevent a man from working when he needed rest. Just as a muscle is kept from working, not by a mere decrease in the power of its tissues, but by the presence of certain poisons formed as by-products, so the mind may be kept from working not by mere inability to work so hard, but by certain outside influences, *e. g.*, feeling bored, sleepiness, headache, which are results of its activity. If the latter were the case, the inhibition of work would probably come by fits and starts, as these outside influences reached summation points, became poison doses, so to speak, and would not come in proportion to the work done. A man might work hard mentally for say eight hours and do just as much the last hour as the first. The curve of ability to work during the progress of work might be not a line like *A* but one like *B*, where the various sharp drops show the influence of the by-

products of mental work. It might also be that we could, perhaps not at all wisely, in some way inhibit the influence of these by-products, and then find ability to do mental work undi-

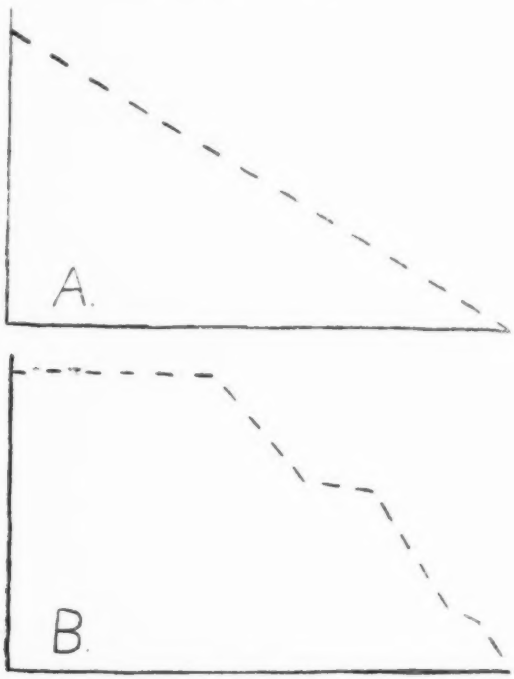


FIG. 1.

minished to any serious extent by the fact of a large amount having been done without rest.

This question seems fairly important, and is of course capable of being settled by experiment. The experiments to be here reported do not settle it, but their results, I think, strongly favor the latter theory.

The method was to test the ability to do mental work before and after periods of hard mental exercise. The only strictly novel test of mental ability was mental multiplication of numbers of several figures each by one another. The results from this test I report first because it seems a rather good one. It is very hard work, at least it was for the subjects chosen, involves an unremitting and intense concentration of the attention, and

also rapidity and accuracy of the associative processes. The very slightest relaxation or confusion probably greatly affects the speed and accuracy of the work. A very little inability to work would, one would suppose, show itself at once in the results.

The first experiment consisted in having a subject who had done five or six such examples, do two a morning on four mornings after good sleep and about an hour's general mental activity, and, after doing hard mental work for from seven to twelve hours, to do two examples each evening. In a second series the examples given in the evening in the first series were done in the mornings, and those of the mornings of the first set were done in the evening. We thus get sixteen such examples, each done once before a day of mental work and once after, and so arranged that practice favors neither side. Nothing of any particular example was ever remembered on any second trial with it.[1] In each case the subject looked at the numbers to be multiplied ten seconds, then closed his eyes and when he had the complete answer wrote it down. The time taken and the errors in the answers were recorded. In every case the evening examples represented a state of mind which had led the subject to stop work on the ground that he was fit to do no more. To start the work of the experiment was very irksome and required some exercise of determination. The record follows. The subject was myself.

TABLE I.

Subject T. Mental multiplication. *Set A*, given *before* work in the first series and *after* work in the second series of tests.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
a.	370 secs.	190 sec.	0	1
b.	215	360	0	1
c.	345	300	1	0
d.	235	340	0	0
e.	300	340	0	1
f.	345	255	0	0
g.	300	380	1	0
h.	440	200	1	0
Total time	2550	2365	Total mis., 3	3

[1] The following are representative examples: 762 x 649, 346 x 782, 279 x 864, 964 x 728.

Set B, given *after* work in the first series and before work in the second series of the tests.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work	Done after work.
i.	420	285	0	1
j.	310	340	0	1
k.	205	450	0	0
..	205	255	0	1
m.	215	250	0	2
n.	260	195	0	1
o.	400	305	0	0
(97 × 68) p.	65	60	0	0
Total time	2080	2140	Total mis., 0	6
Total for both sets.	4630	4505	3	9

The next table records the results of a similar experiment with a different subject, a college freshman, 16½ years old.

TABLE II.

Subject E. T. Mental multiplication. *Set A*, given as in Table I.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
a.	4 m. 30 sec.	2 m. 45 sec.	0	0
b.	3 " 36 "	3 " 10 "	0	2
c.	3 " 45 "	2 " 15 "	1	1
d.	5 " 40 "	4 " 40 "	0	0
e.	4 " 20 "	3 " 00 "	6	0
f.	4 " 30 "	3 " 24 "	3	0
g.	3 " 10 "	2 " 06 "	2	1
h.	2 " 36 "	2 " 12 "	4	2
Total time	30 m. 37 sec.	23 m. 32 sec.	Total mis., 16	6

Set B, given as in Table I.

Examples	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
i.	3 m. 30 sec.	4 m. 15 sec.	1	1
j.	4 " 12 "	3 " 20 "	0	2
k.	2 " 45 "	4 " 40 "	0	0
l.	3 " 20 "	3 " 02 "	4	0
m.	1 " 50 "	4 " 00 "	6	6
n.	3 " 00 "	3 " 20 "	0	2
o.	2 " 15 "	3 " 24 "	1	3
p.	40 "	1 " 30 "	1	1
Total time	19 m. 42 sec.	23 m. 31 sec.	Total mis., 13	15
Totals for both sets.	50 m. 19 sec.	47 m. 3 sec.	29	21

The mental work done on the days when tests were taken was perhaps not so severe in this case. It ranged in amount from five to twelve hours, and averaged nearly eight hours.

The next table (No. III.) records the results of a similar experiment with a little girl eleven years old as subject. The examples were in this case done before going to school in the morning and immediately after return from school in the afternoon, *i. e.*, after an interval of seven hours, five of which were spent in school. The examples in this case were easier, the following being representative: 42×18, 45×56, 74×47, 87×69. Four were given in each experiment instead of two. The subject had previously done eight such examples, though somewhat easier ones.

TABLE III.

Subject M. T. Mental multiplication. *Set A*, given as in Table I.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
1, 2, 3, 4	8 m. 32 sec.	5 m. 10 sec.	1	0
9, 10, 11, 12	7 " 57 "	5 " 30 "	0	2
17, 18, 19, 20	10 " 00 "	9 " 04 "	0	1
Total time	26 m. 29 sec.	19 m. 44 sec.	Total mis., 1	3

Set B, given as in Table I.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work	Done after work.	Done before work.	Done after work.
5, 6, 7, 8	7 m. 30 sec.	9 m. 40 sec.	0	2
13, 14, 15, 16	5 " 28 "	6 " 25 "	4	4
21, 22, 23, 24	7 " 18 "	9 " 00 "	1	8
Total time	20 m. 16 sec.	25 m. 05 sec.	5	14
Totals for both sets.	46 m. 45 sec.	44 m. 49 sec.	6	17

The interpretation of these data will come in more suitably after certain others have been presented. As regards the method of solving the examples, each subject worked at the utmost speed consistent with conscious accuracy. In all cases feelings of boredom or fatigue were reported at the start of the late experiments.

In the case of the last subject, M. T., it may well be that these feelings caused less effort. In the case of the other two, so far as introspective results enable one to judge, these feelings seemed to be set aside and the work seemed to be done as well as possible.

To supplement these tests, examples in addition were given after rest and before work and after hard work before rest. The following is a fair sample of one of these examples:

95799
86967
32687
84799
95976
34797
97864
98945
87834
68792
79867
88896
97745
39799
48970
89043
67354
54628
91176
90251

No two were used alike.

As in the multiplication examples, the subject worked as fast as he possibly could without consciously making mistakes. The time taken and the number of mistakes in the answer were recorded in each case. Table IV. shows the records for myself. It will be observed that the records vary very much in point of speed. That is because during the period covered by the experiments I was doing similar examples for other reasons. Of the exact amount of this practice at each date I have no record. But the reader can be sure that neither side was much favored by it, though the 'before work' ones may have had enough advantage here to account for their superior quality. The

mental work on the days when evening tests were taken was approximately the same amount as in the multiplication experiments of Table I.

TABLE IV.

Subject T. Addition.

Examples.[1]	Date.	Time Taken.		Mistakes in Answer.	
		Done before work.	Done after work.	Done before work.	Done after work.
a.	Feb. 26.		60		1
b.	" "		55		2
i.	Feb. 28.		95		0
j.	" "		114		0
f.	Mar. 3.		94		0
d.	Apr. 25,		75		0
k.	" "		70		2
c.	May 3.		55		2
l.	" "		63		1
4	" "		73		0
k.	Mar. 1.	72		3	
l.	" "	98		1	
c.	Mar. 3.	63		0	
d.	" "	78		2	
f.	Mar. 4,	70		0	
4	Mar. 7.	88		0	
a.	May 4.	45		0	
b.	" "	35		0	
i.	May 24.	50		1	
j.	" "	60		0	
Totals		659 secs.	754 secs.	7 mist.	8 mist.

Table V. shows the results of a similar experiment with Miss K., a college senior.

Besides the mental multiplication and the addition, the speed and accuracy of the perception of small differences was used as a test with Miss K. and T. (myself). Twenty-four small cards were used, each having on it a line. These lines were of lengths from 15 to 30 millimeters, sixteen of them forming an arithmetical progression and the other eight being duplicates of some of them. The subject was required to place these in order according to the length of the lines. Record was kept of the time taken and the number and degree of misplacements.

[1] a and b were examples of only 60 figures instead of 100.

TABLE V.

Subject, Miss K. Addition.

Examples.	Date.	Time Taken.		Mistakes in Answer	
		Done before work.	Done after work.	Done before work.	Done after work.
a.	Mar. 1.		3 m. 30 sec.		0
b.			2 " 00 "		0
c.			4 " 00 "		2
d.			3 " 45 "		0
e.	Mar. 1.	2 m. 45 sec.		2	
f.		4 " 00 "		3	
g.		3 " 00 "		1	
h.		3 " 45 "		1	
i.	Before work. Mar. 19.	1 " 45 "	3 " 00 "	0	0
j.		1 " 50 "	2 " 30 "	0	0
k.	After work. Mar. 2.	1 " 35 "	3 " 00 "	1	2
l.		2 " 00 "	3 " 30 "	1	0
m.	Before work. Mar. 1.	2 " 00 "	2 " 05 "	0	6
n.		2 " 00 "	3 " 00 "	2	0
o.	After work. Mar. 18.	2 " 10 "	2 " 00 "	1	1
p.		2 " 05 "	2 " 10 "	0	1
q.	Before work. Mar. 20.	2 " 10 "	2 " 45 "	2	0
r.		2 " 25 "	3 " 10 "	1	4
s.	After work. Mar. 4.	1 " 50 "	2 " 30 "	1	1
t.		1 " 45 "	2 " 05 "	0	1
u.	Before work. Mar. 5.	2 " 05 "	2 " 05 "	1	1
v.		2 " 10 "	2 " 30 "	0	0
w.	After work. Mar. 19.	2 " 10 "	2 " 45 "	1	1
x.		2 " 05 "	2 " 20 "	0	1
Totals		45 m. 35 sec.	54 m. 40 sec.	18	21

NOTE.—It will be observed that the 'Before work' tests have a slight advantage in point of practice. Subject K. had pains in her eyes in the evening frequently.

Miss K. did this before work on March 3d and March 4th with the following results:

	Time.	No. of Displacements.	Total Displace'ts
March 3d	300 sec.	7	9
" 4th	260 "	8	9

On March 14th and March 15th the same subject did the same thing *after* the day's work. The record is:

March 14th	300 sec.	6	6
" 15th	280 "	5	6

The subject T. did it before work February 28th with the following results:

220 sec.	7	9

After work, April 25th, with the following results:

344 sec.	5	5

The experiments recorded so far have shown the influence or lack of influence of mental work of a general nature on the ability to do mental work of a particular sort. In order to get additional data on this point and also some data about the influence of mental work of a particular sort on the ability to do that *same* sort of work, experiments of another sort were conducted. In these the subject did for some time mental multiplication or ordinary addition, and the rate and accuracy of his work in the early part of the time was compared with that of his work during the latter part. In some of these experiments data to compare with those already presented were gained by giving simple tests of general ability to do mental work before and after the special work.

Table VI. presents the results of two and one-third hours' unremitting work at mental multiplication, the numbers to be multiplied being of four figures *each*, *e. g.*, 8967×4532. I., who was the subject, had previously had the practice of experiment 1. The experiment began about 3 P. M. I had worked three and a half hours in the forenoon, and twenty minutes just before starting the experiment. For certain reasons my emotional tone was very pleasant.

TABLE VI.

Subject T.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
	min. sec.			min. sec.	
1	12.10	2	6	17.30	2
2	10.20	1	7	15.50	2
3	19.50	0	8	14.20	0
4	12.40	1	9	11.50	0
5	16.00	0			

It will be observed that whether one compares the first fourth, third or half of the work with the last fourth, third or half respectively, one finds only very slight differences. Thus, the first two examples took 22 min. 30 sec. with 3 mistakes, the last two 26 min. 10 sec. with no mistakes; the first three 42 min. 20 sec. with 3 mistakes, the last three 42 min. with 2 mistakes; the first four 55 min. with 4 mistakes, the last four 59 min. 30 sec. with 4 mistakes. Yet the effort of holding the at-

tention to this work during the latter period was to consciousness considerable. The later examples were to persons doing them out on paper of the same difficulty as the earlier.

Table VII. records the results of a similar experiment with numbers of four figures each, the subject being E. T. who had had the practice in multiplying mentally shown in Table II. This subject proved very capable at this work, and it was perhaps not so hard for him as for T. Though he did ten examples of the same difficulty as those of Table VI., he worked only one hour and ten minutes at it. He started at 8:30 A. M., after eleven minutes of work.

TABLE VII.

Subject E. T.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
	min. sec.			min. sec.	
11	6.45	2	18	6.40	1
12	5.45	4	19	6.40	0
13	5.30	1	22	7.00	2
15	7.20	1	23	6.00	0
16	7.20	1	24	7.00	2

The first five examples thus took 32 min. 40 sec. with 9 mistakes, the last five 33 min. 20 sec. with 5 mistakes. The totals for the first four and last four are 25.20, 8 mistakes, and 26.40, 4 mistakes; for the first and last three they are 18 min. with 7 mistakes and 20 min. with 4 mistakes.

Table VIII. shows the results during 1 1–5 hours of unremitting addition. The examples were of the same difficulty as the one printed. The subject was T. (myself). I had had the practice shown by Table IV. up to March 7th and its preliminary, and had also done some eight of these examples for another purpose. I had worked four hours before luncheon, and did this experiment because I felt unable to do any regular work. The work was begun at eight minutes of three after thirty minutes of other work. Luncheon and a rest filled the time from 12.30 to 2.20.

If we compare the work of the first eight examples with that of the last eight, the totals are 15.25 with 4 mistakes for the

TABLE VIII.

Subject T. Addition.

Example.	Time taken.	Mistakes in answer.	Example.	Example.	Mistakes in answer.
	min. sec.			min. sec.	
5, 6, 7, 8,	8.10	2	25, 26, 27, 28	6.45	3
9, 10, 11, 12	7.15	2	29, 30, 31, (32)[1]	5.56[1]	0[1]
13, 14, 15, 16	6.25	2	2, 3, 4	5.15	1
17, 18, 19, 20	7.20	2	103, 104	3.30	0
21, 22, 23, 24	8.50	0			

former, and 14.41 with 1 mistake for the latter. Taking the first and last twelve we have 21.50 with 6 mistakes and 21.26 with 4 mistakes. Taking the first and last sixteen we have 29.10 with 8 mistakes and 30.16 with 4 mistakes.

On March 28th after three hours' work and an hour and a half for luncheon and rest I did eight examples of this same sort at intervals during an hour and a half of mental work (computing errors in experiments and recording results). The records are as follows:

TABLE IX.

Subject T. Addition.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
e.	105 sec.	0	t.	95 sec.	0
d.	83 "	0	p.	70 "	0
f.	80 "	0	u.	80 "	0
s.	85 "	0	x.	80 "	1

Beginning at 9.20 P. M. of the same day after a rest from 3 P. M. to 9 P. M., I did at intervals during an hour of mental work nine more examples. The records follow:

TABLE X.

Subject T. Addition.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
r.	80 sec.	1	o.	80 sec.	0
w.	95 "	1	k.	75 "	2
v.	84 "	0	g.	72 "	0
m.	90 "	0	h.	75 "	0
n.	90 "	1			

[1] The record of 32 was lost, so that I have taken as the time for 29, 30, 31, three-fourths of the time recorded for doing all four.

Comparing the first four with the last four we have 5 min. 49 sec. with two mistakes and 5 min. 2 sec. with two mistakes.

Before and after the mental work recorded in tables VI., VII. and VIII., various tests of ability to work were given. A statement of the results they manifest will complete the presentation of the data concerning the amount and manner of influence of work done on the ability to do work.

Before the mental multiplication of Table VI., addition examples q, r, s and t were done, and after it examples u, v, w and x. The record of the early set is 422 seconds with no mistakes; of the late set 407 seconds with 7 mistakes. Before and after the mental multiplication of Table VII., E. T. did example 1 (addition) and sorted the lines mentioned on page 457. After the mental multiplication he did example 2 and again sorted the lines. The records are for the early addition 3 min. 45 sec. with 2 mistakes; for the late 3 min. 30 sec. with 1 mistake; for the speed and accuracy of perception the early test took 4 min. 45 sec. with 8 mistakes, involving ten degrees of displacement; the late test taking 3 min. 50 sec. with 10 mistakes, involving ten degrees of displacement. Before and after the addition work of Table VIII., subject T. was tested for the time of recognizing and naming the cards in a pack, for the time of sorting a pack into piles of aces, deuces, etc., for the speed and accuracy shown in marking on two pages of practically equal difficulty all the words containing the letter e, and for the speed and accuracy of mental multiplication.

The results were as follows:

	Before work.		After work.	
Recognition and naming.	65 sec.	0 mist.	56 sec.	0 mist.
Sorting cards.	120 "	0 "	104 "	0 "
Multiplication.	462 "	0 "	695 "	0 "
Marking words.	180 "	161 marked, 6 omitted.	173 "	160 marked. 2 omitted.

To judge what part the practice of the first trial played, the subject seven weeks later performed similar tests, but separated this time by a period of rest. The times for recognition were 56 and 56, for sorting the cards 110 and 106, for marking words 165 and 158.

Now the conclusion which all these data strongly suggest is that the fact of relative incompetency is not to be explained as the result of a decrease of mental energy in general. Mental energy, if it means anything, must mean a something which mental work uses up in regular proportion to the work done. But incompetency, mental fatigue, *does not come in regular proportion to the work done.* In general it comes very much more slowly. A great deal of mental work can be done with very little change in the ability to work. Thus Table I. shows that although there were over 400 chances to make mistakes in the associations involved in multiplying, besides all the chances to forget the figures (about 18 per example) and the positions of each of those figures, yet only six more were made in the work done late than in that done early, and the work was done *more* quickly. Table II. shows no decrease whatever in the ability to work due to the work done. Table III. shows that with over 300 chances to make mistakes in associations and corresponding chances to forget the figures and their positions, only eleven more were made in the late set, and *less* time was taken. Table IV. *may* show nothing more than the change in ability due to practice or a slowness in recognition due to the inferiority of artificial illumination, or the physical fatigue of the sense organs. If these factors play no part, there is a clear, though slight, decrease in speed and accuracy. Table V. shows a clear decrease in speed, which, however, may be referable to difficulty in vision during the late tests. Tables VI., VII., VIII., IX. and X. show that the decrease in energy does not have enough influence to outweigh the influence of practice.

We see also that the change in ability to work is not in any regular proportion to the work done. If, for instance, E. T. in test 11 had worked twenty-four hours instead of eight, he would in some cases undoubtedly have been entirely unable to multiply three figures by three mentally. He would have fallen asleep or become mentally confused or have failed to keep in mind the first half of the work while doing the second half. Eight hours' expenditure of mental energy had no influence on his ability to do the work, while twenty-four hours would have almost destroyed it.

The fact seems to be that mental fatigue is a very complex affair; that it is not a simple matter of less mental energy but a name that stands for slowness of associations, inaccuracy or inappropriateness of associations, lack of inhibition or irrelevant ideas, interference with purposive trains of thought by feelings of ennui, of bodily pain and strain, thoughts of pleasant activities, impulses to stop work, mental confusion, sleepiness, physical fatigue of the eyes, and perhaps a number of other things. It seems to the present writer that the most hopeful way to attack the problems of mental fatigue is to resolve the fact into such elements and to study each by itself. For instance, the influence of certain kinds and amounts of work on the speed of associative processes is easily estimated. And so with the influence of work on the other factors. Until one so discards the notion of general mental fatigue, one's questions and results are largely meaningless, for they are so vague. Fatigue of a muscle is a definite thing, viz, relative inability to overcome resistance, but fatigue of the mind means inability to do the right *kind* as well as the right quantity of work, and may refer to sonnet-writing, logical theorizing, sharpness of observation or a hundred other different sorts of things.

So much for the *fact* of fatigue. Our second conclusion that there was no pure feeling of general mental incompetency is, of course, the result of personal introspection, though under test conditions. When I tried to analyze my feelings during states which in accord with the social consciousness I called feelings of mental fatigue, of inability to do mental work, I found in them emotions of repugnance at the thought of certain forms of mental activity, amounting sometimes to a sort of mental nausea; feelings of dulness or stupidity (by which names I mean a state of unsuggestiveness, of insipidity), cravings for certain familiar forms of mental relaxation, feelings of sleepiness, heavy feelings in the head, pains in the chest or back (from leaning against a table and sitting upright during work) and sometimes a feeling best characterized by the awkward phrase mental 'goneness,' which reminds one of the feeling of physical faintness. This last is most likely the supposed feeling of incompetency, but I get it only very rarely and not

necessarily after especially hard mental work. I fancy that it has some direct physical cause. I was constantly surprised to find myself when feeling, as I would certainly have said, 'mentally tired,' unable to demonstrate in the feeling anything more than an emotional repugnance to the idea of doing mental work. On at least half the occasions this seemed to be all there was.

If this account of the feeling of fatigue is correct it corroborates the previous account of the fact of fatigue. If the latter were the complex of conditions we supposed, we should not expect that there would be any pure feeling of mental inability. The introspection on which I have based the account may, however, have been influenced by suggestion, and prejudiced in favor of the theory involved.

Our third conclusion was that the feelings of fatigue, such as they were, were not measures of mental inability. Kraepelin has emphasized this fact, but chiefly in connection with the claim that we can be mentally fatigued without feeling so. What I wish to emphasize is that we can *feel* mentally fatigued *without being* so, that the feelings described above serve as a sign to us to stop working long before our actual ability to work has suffered any important decrease which an experimenting psychologist could measure and use as a warning to us. The greater number of the 'after work' tests described in this paper were undergone when the subject would, if left to himself, never have engaged in mental work. He did them often *just because* he felt incapable of mental work. I have myself when feeling thoroughly tired summoned up courage enough to start the repugnant work of multiplying and found that I did it just as well as ever. The young subject E. T. noted this independently and wrote me, "Mental fatigue is all a fraud. I don't want to do the things at night at all but I can do them just as well." Children in the schools where I gave tests early and late in the day agreed that they were tired in the late hours and thought that they couldn't work nearly as well, yet these same children *did* do just as well in the tests given. The relation between the feeling and the fact of fatigue then is not a simple relation of direct dependence but a very involved rela-

tion or series of relations. These feelings may be in part warnings, preventatives of overwork, provided as a beneficent mental adaptation. They may be in part mere useless by-products of our thinking to which mental hygiene need pay no attention. They may even be in part mere accidents of thinking which habitual disregard may abolish. To find out just which of them belong in these or other categories would be a worthy task, for the wise expenditure of one's mental effort depends largely on the way one interprets these symptoms.

The gist of this whole article, then, is the claim that certain current ideas about the fact and the feeling of mental fatigue, and the relation between them, are naïve abstractions based on a simple-minded analogy, a failure to carefully analyze certain mental states, and a confusion in the case of experimental investigations between lack of *desire* and lack of *ability* to work.

A TYPE OF PENDULUM CHRONOSCOPE AND ATTENTION APPARATUS.

BY PROFESSOR JOHN A. BERGSTRÖM.

Of the pendulum chronoscopes which have been constructed for psychological purposes successively by Professors Sanford,[1] Fitz,[2] Scripture[3] and Seashore,[4] the one here described was more especially suggested by the Fitz-Scripture type. Like this it secures the registration of the intervals by the mechanism of a heavy pendulum carrying a light index, which may be clamped at any point of a scale graduated into hundredths to thousandths of a second.

Aside from the circular mercury cup attachment and a few details of construction, it differs from it chiefly in the mode of carrying and clamping the index. The mechanism of this part is exhibited in cross section in figure I, and in a front view in figure II. The same parts in the two figures are referred to by the same letters.

The steel axis $X^1X^2X^3$ is cut so as to leave the solid sectors K^1 and K^2, which have an angle of about 30°, in line and with knife edges coincident with the longitudinal center of the axis. These knife edges rest in shallow V-shaped grooves of hardened steel on the vertical supports V^1, V^2, and the parts are so proportioned as to allow the pendulum to swing through the necessary arc without jolting. The axis, which rotates as it would on any other centered bearings, is cut down to a smaller diameter in the forward projecting section X^1 and carries there

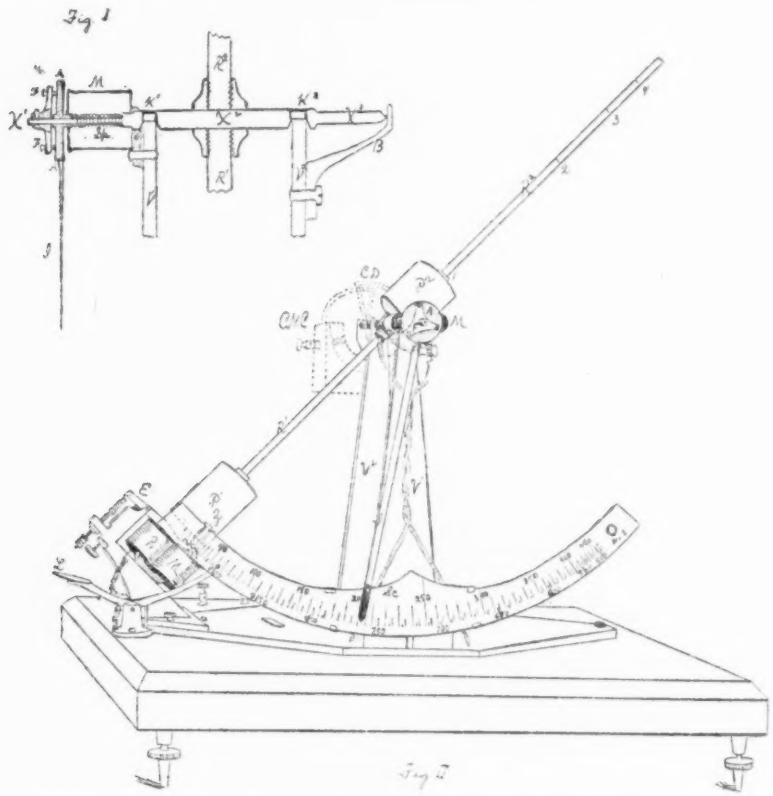
[1] E. C. Sanford, A New Pendulum Chronograph, *Am. Journal of Psychology*, Vol. V., No. 3, April, 1892. Also, The Vernier Chronoscope, Vol. IX., Jan., 1898.

[2] G. W. Fitz, A Location Reaction Apparatus. PSYCHOLOGICAL REVIEW, 1895.

[3] E. W. Scripture, Studies from Yale Psychological Laboratory, Vol. III., 1895.

[4] C. E. Seashore, Spark Chronoscope, University of Iowa Studies in Psychology, Vol. II., 1899.

the essential parts of the recording mechanism. This consists of the circular armature *AA*, which may slide forward and



backward or rotate freely on the axis X^1 and has the index *I* attached to its circumference. The spiral spring *Sp* holds it against the tripod *F*, which serves to adjust it to the proper distance from the electromagnet *M* and to provide the friction necessary to carry the index along with the pendulum as it swings.

If a current of sufficient strength is sent through *M*, the circular armature *A* is at once attracted and moves from the friction tripod *F* to the magnet, by which it is held immovable as long as may be desired—the pendulum, in the meantime, swinging on with little resistance till it is caught on its return by one of the lower teeth of the release armature *E*. The

backward pressure exerted by the spring *Sp* on the axis when the armature *AA* is thus clamped is checked by the brace *B*. The lever *L* serves to move the pendulum up to its starting point, while the index is set back to zero by moving it against the projecting pin *Z*.

The time of a process is measured by sending one current through the release magnet *RM* at its beginning; and another through the index magnet *M* at its end. Both the release of the pendulum and the clamping of the index armature are thus brought about by the closing of the circuits; and the currents may always be kept in the same direction, so that there is no danger of any error from residual magnetism.

This, of course, does not necessitate the use of make-circuit keys in experimentation, but break-circuit keys may be employed as well, if they are used as shunts in the circuits of the magnets. The latter method should, in fact, be adopted in all work requiring a high degree of accuracy on account of the somewhat variable lengthening of the time, due to make-circuit keys. Break-circuit keys, however, while less objectionable, are not wholly so, as variations may be introduced by different tensions of the spring acting on the lever, and the rate and force of the movement of the arm.

In constructing the scales, use was made of the well-known spark method for registering intervals. A fine wire was run down the index so as to leave a point projecting at its end to within a millimeter or less of the metal plate on which the scale is fastened. The front surface of the metal plate was then covered with smoked paper moistened so as to adhere closely, and the sparks from an induction coil interrupted by a hundred-vibration tuning fork with a mercury contact were allowed to run over it as the pendulum swung from one end to the other.

Since the pendulum might start at any point between two sparks, successive spark tracings would not coincide, and the number of sparks up to any point on the scale would not, except by chance, represent the absolute time. Moreover, it would not be possible to construct the scales for the return swing, of which use is made in this instrument, on account of the confusion of records. To obviate the trouble and secure the ad-

vantage of successive coincident tracings, five or six of which were imprinted on the copies for the scales made here, the starting of the pendulum was made dependent upon the dipping into the mercury of the same vibrating point of the tuning fork that broke the circuit of the induction coil for the sparks. By reducing the depth of the dip into the mercury the time between the making of the circuit which releases the pendulum and the breaking of the circuit which gives the spark may conveniently be made about two thousandths of a second, an interval for which compensation can easily be made by the mechanism of releasing the pendulum and clamping the index so that the instrument will register the absolute time.

When the forward scale has become well fixed by half a dozen tracings and has besides been marked, the return scale can be constructed without confusion.

Relative variations in the strength of the currents operating the two magnets produce variations in the records. The instrument must therefore be adjusted for a certain ratio, and the most convenient is, perhaps, that of equality. If the currents are equal, considerable differences in the absolute strength, as from one-half to one and one-half ampères, do not seem to affect the record appreciably, though a current of one ampère is usually employed.

The chronoscope has four scales numbered from one to four and corresponding with times of vibration of about one, one and one-half, two and three seconds. The different times of vibration are produced by placing the counterpoise P^2 at points 1, 2, 3 and 4 on the rod R^2. For the usual experiments upon the duration of mental process, scales 1 and 2 or 4 have been found most serviceable. With 4 the forward scale is usually sufficient for the longer reactions, but the return scale of number 2 by giving greater distances for the intervals can be read with greater precision and may be used for the same purpose, and there is less difficulty in securing the proper adjustment of the counterpoise. The counterpoise is eccentric with respect to R^2 so as to make it possible to give the pendulum the proper balance in each case; and upon this and the proper leveling of the base depends the securing of the absolute time.

For regulating and learning how to manipulate the instrument use was made of a falling ball apparatus, like one described several years ago by Professor Jastrow.[1] In this, an iron ball about three-fourths of an inch in diameter is held suspended by a short, straight electro-magnet. On the interruption of the current the ball falls and is made to strike a break-circuit key, which may be placed at different distances below the magnet. This break-circuit key serves as a shunt in the circuit of the index magnet, so that opening it leads to the clamping of the index. The time required by the ball to fall to the key is then compared with the readings of the chronoscope. To make the release of the ball and the starting of the pendulum simultaneous presents considerable difficulty, aside from that due to the inertia of the magnet; and two kinds of keys were used for the purpose, one like that constructed by Professor Jastrow with double levers, in which the striking of the first upon the second makes one current, while the lifting of the second from the impact of the first breaks another, and a second in which two currents were broken by the lifting of the same long lever from two insulated points of contact. The second approximates most closely to simultaneity, as the intervals recorded are about .001^s longer when the first is used, and the mechanism is such that the circuit for the release of the pendulum is made first in the double lever key.

The manipulation of these keys represents one of the chief sources of error, as small differences could be produced by varying the rapidity of release.

At points in the scale where the thousandth of a second was represented by about a millimeter, and the scale was so marked, the readings in tens of thousandths being estimated, the writer found that the probable errors of single records deduced from groups of fifty successive records each were for intervals of about .200 and .300^s, $\pm$ .00021 and $\pm$ .00012 respectively. The difference in the probable errors is perhaps due to improvement in manipulation, as the two groups of records were taken in succession, though small errors in the estimation of the ten-thousandths must occur. The corresponding average

[1] Jastrow, *American Journal of Psychology*, Vol. IV., p. 208.

variations are, .000168^s and .000119^s, and the average variation of the instrument for the two series together is .000144^s.

In comparison, the average variation of the Hippchronoscope tested by Professor Jastrow by the same method was .002^s, or nearly fourteen times as great; and Professor Sanford gives .002^s as the lowest average variation of the pendulum chronograph.

The accuracy of the other instruments is not stated definitely. Professor Scripture says that the average error of the chronoscope constructed at Yale is not more than .002^s, and Professor Seashore, that the value of the average deflection in the striking of the spark in the spark-chronoscope amounts to less than $\pm$ .001^s when the divisions into hundredths equal 5 mm., and varies inversely with the lengths of these divisions.

I am indebted to a group of three students for the following probable errors for single records from groups of fifty records each for the intervals .100, .200, .300 and .400^s with scale I., namely, $\pm$ .00014, $\pm$ .00016, $\pm$ .00022 and $\pm$ .00015^s respectively; and for intervals .200 and .400 with scale II., $\pm$ .00013 and $\pm$ .00024^s, the last group being taken hastily at the end of the period. In this case the estimate into ten-thousandths is, perhaps, less certain, because it was done by the deviation of the point from the center of the dividing lines on the scales, while the scales were marked into only two-hundredths. The correspondence with the previous records, however, is sufficiently close.

Another feature of the instrument is represented by the circular mercury cups *CMC*, and the circular dips *CD*, which are only attached for certain purposes and are consequently represented in dotted outlines. The dips *CD* can be moved around the axis X^3 so as to make or break a circuit at any point of the swing of the pendulum, or to make or interrupt a circuit for different intervals of time. For the measurement of intervals less than .050^s they may be used to transfer the readings to the central part of the scale, but they come especially into play when the instrument is used for the study of attention to simultaneous stimulation of vision and other senses. As in Wundt's[1]

[1] Wundt, *Physiologische Psychologie*, 4th Ed., Vol. II., p. 405.

apparatus, the moving index supplies one stimulus while the others are brought about by electrical means on the closing of the circuit through the mercury cups. The time errors can be determined directly from the scales, though for extensive experiments more scales than the instrument now possesses would be needed.

The use of the mercury contacts is in some respects inconvenient. Without constant care the accumulation of mercurous oxide and other materials, and to some extent the lack of constancy in the surfaces, particularly on breaking contact, will give rise to large variations in the records, and even with fair care the probable errors of the instrument are two or three times as great as when they are not employed. Thus for breaking circuit with the mercury covered with a layer of either alcohol or kerosene, the probable error for a single record in a group of fifty was $\pm$ .0006". The addition of the alcohol or kerosene makes the circuit break three or four thousandths of a second more quickly. The contacts were cleaned at the beginning and again at the middle of the series. The probable error in the case of the making of the circuit was $\pm$ .00039". The surfaces and dips were cleaned at the beginning and four times throughout the series, but no alcohol or kerosene was used. The variation in the latter case is due not so much to irregularity as to the fact that from one cleaning of the contacts to another there is a progressive lengthening of the record. The average of this group would therefore be quite reliable as the starting point for the measurement of intervals, and a rather high degree of accuracy might be attained by making due allowance in other cases.

The instrument was constructed about three years ago in the workshop of the psychological laboratory of Indiana University, and has proved very convenient. It can be operated more quickly than it is profitable to make the usual experiments.

DISCUSSION AND REPORTS.

PROFESSOR JUDD'S ILLUSION OF THE DEFLECTED THREADS.

In the number of this REVIEW for May, 1898, Professor Judd described a novel optical illusion, the peculiarities of which he regards as confirmatory of a particular doctrine of space perception. So far as I know, Professor Judd's proposed explanation of the matter has thus far stood unchallenged, for the reason, perhaps, that readers of space literature are more and more well disposed towards the general position that his conclusions represent. The essentials of the illusion and its explanation may be briefly recalled. Two threads, lying in different horizontal planes, are stretched across a box in such a way that their intersection forms an acute angle. The line of sight of the observer should form an acute angle with the planes of the threads. Under these circumstances there are obviously two points of intersection of the threads, one for each eye. Alternately closing either eye will reveal the location of these two monocular points. It is in the region between them that the following illusion occurs. With both eyes open, choose a fixation-point midway between the two threads, in about the middle of the region just mentioned. If this point of fixation be properly chosen, two illusory threads will suddenly appear lying at the right and left respectively in *vertical* planes and thus joining the two actual threads. Their slopes are not identical. One is nearly perpendicular, the other inclines strongly down and away. If viewed from the side they would appear in the form of a somewhat distorted X. The explanation offered is this: Each eye alone sees a pair of intersecting threads without being able to perceive any difference of depth between them. For a single eye the threads appear to lie in one plane and cross at a point peculiar to that eye. Binocularly, however, there are differences of depths perceivable. With both eyes open, therefore, there are two monocular points in the binocular field. Accordingly vision is under the necessity of somehow adjusting itself satisfactorily to both sorts of demands, the monocular as well as the binocular. The result is the two illusory threads running through the third dimension and connecting the mon-

ocular crossing points. The theoretical deductions are wholly along the line of emphasizing the importance of *binocular* factors in all visual perceptions of depth. Impressions that are strictly *monocular* have no definite meaning in terms of distance, whether relative or absolute.

Now while I am in most hearty accord with the general position towards space perception that Professor Judd has expressed both here and in other papers, I cannot see my way clear to accept the connection that he makes between this particular illusion and his theoretical comments. For I have recently become convinced that the experimental analysis of the illusion was, if I understand it, deficient in several critical points. And, unless I am greatly deceived, a much more simple and straightforward explanation than the one originally given must be accepted.

In the first place, inclination of the line of sight to the planes of the threads is not at all necessary. In fact the definiteness of the illusion is perhaps greatest when the line of sight is perpendicular. And, what is vastly more important, *there is absolutely no necessity that the monocular crossing points be in the field of view at all.* Two strips of black cardboard may be so placed upon the edges of the box in which the threads are stretched that the two eyes may clearly see the region between the two monocular crossing points while these points themselves are screened from view. The illusion is now only the more vivid. Yet under these circumstances there can be no thought of a necessary adjustment to two sets of relatively conflicting factors. The cause of the illusion must be sought elsewhere. I find this cause to lie solely in the particular fusions of images which take place when the point midway between the threads is fixated. What happens may perhaps be most clearly described by noting what changes take place in the binocular images as the point of fixation is raised from a position *below* both threads. The following figures represent these successive changes. Let the threads be stretched so that the upper one runs from left to right, the lower from right to left. Let a and b designate the images of the two threads received by the right eye, c and d those received by the left eye. Whether the image comes from the upper or the lower thread will be indicated by the letters u and l respectively. Finally let R and L be the inclusive or single designations for what is seen by right and left eye respectively.

Fig. 1 presents the case where the point of fixation is below the level of both threads. There is no fusion of double images. The pairs ab and cd are upon disparate points, and of course four sepa-

rate impressions are received, the double-images being of the 'crossed' variety. In Fig. 2 the fixation-point is on the lower thread. The images *b* and *c* have united. Crossed double-images of the upper thread remain. In Fig. 3 the fixation-point is raised to the upper

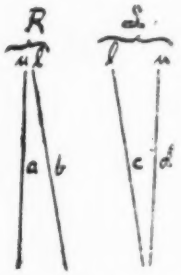


FIG. 1. Fixation-point on bottom of box.

FIG. 2. Fixation-point on lower thread.

line, whereupon *a* and *d* fuse. Uncrossed double-images of the lower line remain. In Fig. 4 is depicted the state of things when the fixation-point is *between* the threads.[1] *a* and *d* have not come near enough to fuse, as in Fig. 3, while at the same time *b* and *c* no longer fall upon corresponding points, as in Fig. 2. The entire process due to the successive fixations is, of course, to be pictured first as an approach, then as an over-lapping, and finally as a crossing over of the two monocular fields. Now Fig. 4 gives the stage of most perfect *over-*

FIG. 3. Fixation-point on upper thread.

FIG. 4. Fixation-point between lower and upper thread.

lapping. Each image of one eye falls obliquely upon an image of the other. Consequently the images must fuse at and about the points of intersection, for the simple reason that the latter represent correspond-

[1] A movable fixation-point within this region may be conveniently secured by introducing a wire, or something similar, through an opening in the side of the box.

ing retinal points. And just in this state of partial fusion of two linear images,—such fusion that crossed double-images remain on one side of the point of fusion while uncrossed double-images remain on the opposite side,—just in this state lie the indispensable conditions for the perception of lines running obliquely towards or away from the eyes. Any straight line (*e. g.*, a piece of wire viewed against a uniform background) will, when held so as to slope obliquely *towards* the eye, present the right hand cross of Fig. 4. A line sloping *away from* the eye will appear as the left hand cross of the same figure. The double-images existing on both sides of the fused point of fixation are often hard to see, but they are none the less there. Running the point of sight along the perceived direction of either illusory line brings about their progressive fusion, exactly as when the eyes pass along an objective line of a similar slope. And herein, of course, lies the ground for any and all perceived linear *directions*. For any such perceived direction means neither more nor less than that the point of sight must necessarily take this course in order to bring the various binocular impressions of the line upon corresponding points. If this view of the matter is the correct one, nothing but the universal laws of tri-dimensional vision are here in operation. Vision is not adjusting itself to novel conditions supplied by the experiment. It is simply behaving as it always does in constantly recurring experiences of everyday life.

Several further facts are to my mind entirely confirmatory of the position above taken. It will be remembered that, when two rows of retinal elements are in *complete* fusion, the resulting perception is that of a line perpendicular to the line of sight. The necessity for such perception under these conditions was, as the reader will recall, the explanatory basis of the interesting illusion described by Mrs. Franklin in Vol. I. of the *American Journal of Psychology*. In fact the illusion here in question is at bottom only a complication of that. Both involve identical principles. But to return. By properly adjusting the line of sight to the planes of the threads, one of the illusory threads may be made to lie perpendicular to the line of sight. As things were arranged in my own experiments, this phantom thread could be made to appear at the *right*. Meanwhile the phantom thread at the *left* assumed a marked slope. And at the same time this latter thread possessed neither the solid color nor the spatial stability of the former. While the former remained steadily there, the latter tended to break up into its components and vanish. The double images of the latter were too obtrusive, though a proper movement of the point of

fixation could successively unite them and keep the line alive. With the line of sight perpendicular to the planes of the threads—the position recommended above—the slopes of the illusory threads are the same, though in different directions. And in this case both threads are seen by careful gazing to possess double-images on both sides of the region of fusion. In fact one is reminded of the appearance of the lines in the stereoscopic view of a truncated pyramid. The double-images are essentially the same in the two cases.

A still further observation adds a strong bit of positive evidence. An entirely new form may be given the illusion by supplying a new objective condition. Stretch a third thread along the bottom of the box and parallel with its sides. Let it lie midway, laterally, between the monocular crossing points of the original threads. We have now three threads at different levels. Fixate the middle one at the proper point, and the impressions from the upper and lower will unite to form the perception of two phantom threads, similar to those with which we are already acquainted, but longer and with a greater distance between them. Here, as in the original case, the various monocular crossing points may be entirely screened from view.

In view of these various considerations I cannot refrain from the conclusion that Professor Judd's illusion presents no new visual principle. To say that it is due to the necessity of satisfying demands imposed by the presence of monocular factors in a binocular field is hardly more than to give a general statement of the conditions under which the illusion is to be seen. It is certainly not a true *explanation* that is thus offered. But however that may be, I am convinced that we have before us in this illusion only a particularly interesting case of what is eternally happening whenever we open our eyes to see.

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VOLITION AS A SCIENTIFIC DATUM.[1]

The writer would willingly be in error in his apprehension of a certain tendency apparent in recent discussions in psychology, which, if real, is unwholesome in its effects upon the development of the science. In many directions, the effort of listening with obedience to the behest of the age that all knowledge and all doing shall be made 'scientific,' seems to be repeated in as many renewed forms. As psy-

[1] Read before the American Psychological Association, New Haven, December, 1899.

chologists we have been defining our composite results for many a day as the science of mental phenomena, just as though these phenomena could not be considered from any other point of view, or with the aid of any other method than that which psychology uses. But our scientific colleagues have been insisting that this staple characterization can continue only through courtesy on their part. Some of us seem to have been struggling to make their politeness good by accepting the implied criticism of what they deem the unscientific character of psychology. The positive changes due to an acceptance of this hint constitute the apparent tendency in certain quarters to whip the science of mind into a definite shape, thus catering to the respect of the critical colleagues, with the hope of making an exposition of the science seasonable to the current progress of science in general.

The potency and scope of this tendency, described in some such general manner, are shown in the reconstruction of the whole subject-matter which is thereby necessarily involved, and actually carried out in some familiar instances. The postulates of explanation and the principles testing the truths arrived at, the intimate nature of the particular method, or methods (if a plurality can be clearly thought of in this connection), the actual factors regarded as the starting-points for explanation in each typical experience, the specific resorts devised in exposing the processes which effect the results known in terms of composite events in consciousness, are some of the particulars upon which the tendency expends itself. In other words, the truths of the science of psychology are forced into a re-making. The elements of conscious experience are to be reduced to those of one kind or quality only. The laws descriptive of the combinations of elements into the total product of any developing process are to be reduced in number and formulated solely with the terms of the fewer elements. Such simplification in the equipment of the science is supposed to render its ascertainments more truthful than are those which fill the psychology which is to be superseded.

It is the aim of this paper to take up briefly one of the points which is profoundly affected by this tendency and the consequences which appear in its train. It is a discussion of volition as a datum for the psychologist, in so far as he is interested primarily in the processes and functions of consciousness, that is proposed. Specifically stated, it may stand thus: Is the psychologist required to recognize volitional appearances in consciousness as constituting an independent aspect thereof; or, can he be justified in resolving volition wholly into certain forms or elements of consciousness which do not primarily possess the

marks of volition even in the form in which he would thus essentially admit it.[1]

In one respect the phenomena of volition present psychology with one, particular, partially isolated, and rather definitely statable problem. It may be found in a number of expressions. What are those descriptions which enable one to pick out volitions from sensations in consciousness? Does a characteristic term, or do varying terms, sum up its psychical nature? How and when does volition arise? Into what mechanical or organic relations with other alleged psychological data can volition enter? Can volition be regarded numerically, *i. e.*, counted in terms of time-rate, or measured in quantity? To what extent and in what manner is volition to be related to cerebral processes in the two-fold time relation of before and after conscious changes? Is volition to be viewed as a problem for psychology only, or can it be made plain through some other form of approach? However many might be the detailed propositions that set forth the problem of volition, it must be put upon a par with the specific problem of sensation or of movement, and must be solved—in so far as the psychologist solves anything by his manner of relating conceptions of elements and processes—with the aid of conceptions appropriate to itself rather than to other forms of consciousness.

On the other hand, volition and its cognate phenomena can be looked upon as constituting the very question of psychology itself. Here is where the tendency under consideration makes its greatest influence felt. For example, the most interesting book of the year just ended makes psychology get along very well as it is argued, without volition and its derivative phenomena. Through this sweeping negation of certain data, psychology is cast into a form different from that which has prevailed hitherto. Does the ability of psychology to continue to exist as a 'science' depend upon its attitude towards facts of a volitional quality? This relation has, indeed, been urged over and over again, in one way and another, in most of the great treatises which have appeared during the last decade.

It must not be overlooked that many writers who support the tendency under consideration do so while *apparently* recognizing the validity of volitional phenomena. This double attitude expresses

[1] The issue can be best brought to the surface by means of the alternative. This need not prejudice one in favor of the 'either—or,' or of the 'neither—nor.' To psychologize is not to deal in mere logical oppositions; the historic struggles lying behind the present achievements of science have been too great for us to allow ourselves, with any sort of family respect, to relapse into such limitation of childish thought.

itself thus: volitional processes may be spoken of, but always with the insistence that their supposed primary marks are, in reality, only those of a 'developed and secondary' phenomenon in consciousness. Indeed, at a much earlier time, the practical outcome of associationism foreshadowed the current tendency in its reduction of volition to a mere aggregation of "ideas of muscular motion with the ideas of those pleasures which the motion produces." And the tendency at present is well expressed in its determination to keep volition securely within the region of 'secondary' phenomena—and thus rejectable—by analyzing it into elements which do not possess, in separation, those qualities which volition is conceded to bear. The elements reduced to one kind are supposed to do all the work for the student by way of making full and satisfactory explanations.

This brings us face to face with one of the chief demands made by the tendency in its notion of what scientific attitudes are in psychology. Can one be justified in lending his efforts to the support of this tendency to eliminate all data which are regarded by some as not readily conforming to the modalities of scientific explanation? Inasmuch as the dominant term in our theme is 'scientific,' let us pause a moment to get a proper conception of what science involves. When one regards the trained student at work with his object, the activity exhibited may be considered in its four phases of aim, material, method, and result. There can surely be no question as to the relative intentness of the student. He has come into relation with his object through a process of selective discrimination. It is a forced and secondary relation, entered into with the aim of getting some sort of knowledge, which shall at least differ from the casual information incidental to the mere passing daily experience of him who is not such a student. To be a scientist is to maintain an assumed attitude towards the content studied.

Our imaginary student is all the while engaged with some 'object.' It need not necessarily be a center of detachable references objectively regarded. What the object may be must be determined by the student, rather than by ourselves as observers of him. In so far as the description of his object is not in conformity with those given by his associates elsewhere, he can be called to account for his digression. The material in any issue cannot be a fabrication of the moment. It is compelled to carry the credentials which can be given by experience alone. Just as soon as the center of scientific activity is shown to be beyond the limits of a given region, other students with like aims are freed from all demands made by him. Naturalness, recurrency,

sequence, and openness to experience, these, at least, are the primary characteristics of the material involved. Indeed, the whole of our present thought is concerned with this particular phase of the tendency which would limit the possible number of objects to which the psychologist can legitimately turn his attention.

The object of inquiry is not merely possessed for the time being by him who studies it, but its possession must have been gained in a definite manner. Facts lying around loosely, guessed at or gathered by every means, cannot be turned over into the precincts of science. Instant appropriation of that which is given in ordinary observation, and the acceptance of such material as comes in due course of advance to objects that are remote, are scientifically approvable when controlled by the aims already mentioned. Mere method of approach and mere method of inquiry, however, cannot alone constitute the one attitude in question. Here the material has a peculiar dominancy which the most artful student is ever forced to recognize. Facts of one order cannot be converted into facts of another order for the purpose of simplifying methods.

Again, the student fulfills his mission only when giving something as a result of his exceptional activity. Here the circle, so to speak, completes itself as his pronouncements show forth the original aims. The real outcome of science, and so far forth the function of science, is the formation of just those judgments about specific 'objects' in which you and I and the unborn thinker in his day may share alike, in terms of appreciation or of revision. In a large measure, the mark of validity of such judgments consists in their sharability. To put it in another way, the result of science primarily is the ability of the student 'to say things' which he has never said before, and which others may say after him. In this wise, the science of psychology finds its business to consist in getting valid concepts and judgments on the basis of those facts which are contained within its sphere.

If the changes in the science, consequent upon the tendency we are considering, are made in sole deference to the achievement of the well-established physical and chemical endeavors of man, then the tendency mistakes formal appearances in results for real values in fact. Mere imitation never discovered an element nor devised the formulation of a law. If, on the contrary, the apparent departures in psychology pronounce themselves progressive because true science must expound itself after one fashion only, and that the fashion of some one or two definite sciences, then all efforts made for this purpose are duped into the acceptance of an epistemological ground-work that has

no appropriateness with reference to the facts that are to be known or made the point of departure in every scientific development. Mere modality in the thought-world should fill the attention of the rhetorician only.

Let us turn to get a correspondingly brief glimpse at the second important item in our heading. What is it for a thing to be a datum of science? What are its marks? How many different kinds of data is the student justified in discriminating when he proceeds to the selection of those in one group, rather than those in another group? There is more danger to science of every order in the forced recognitions demanded at this point than through any other disposition one can assume, unless it be that which breeds shallowness. The general tendency forcing those recognitions grows, to a large extent, out of that mode of thinking which conceives all attempts at science as under the necessity of reducing results to the 'lowest terms' common to all sciences alike. The very suggestion of our theme is explicit with reference to the dependence of a science upon the character of the material which must be admitted into it. To say that psychology presents one with the merely subjective side of that which is objectively given to physics, chemistry or neurology, is a mistaken conception of their interrelation. It is a position which fails to offer any suggestion of newer methods of inquiry, and at the same time renders obscure a possible insight into the higher unities which manifest themselves in the course of conscious developments. How far, might it be dubiously questioned, would a physicist get with his problems should he suddenly propose to push his solutions thereof into the forms of psychological explanation? That the medley he would thus produce could not be accepted by either the physicist or by the psychologist, one could well guess. And we might even raise strong doubts as tc whether psychology changed under the influence of this tendency is any more acceptable to the exact sciences than is the psychology which deals with mental phenomena in its own characteristic manner.

That which forms the content of the student's labor has been called his 'object,' in the most general sense of that term. But, what marks must the object possess in order to be admitted to scientific cognizance? Over against the reply to such a query should be placed the one given in response to this:—What are the literal characteristics of any datum whatsoever? In either instance, it is needful to recognize the intimate relation obtaining between the facts of experience, as such, and those facts which can serve, in any explanatory manner, the aims of the scientific investigator. The first delimitation

of any science consists in the specification of the qualities which a datum must possess before it can arise in the attention as demanding full value in any description or explanation. Can the object be a 'thing,' spatially or objectively regarded? Must the phychologist's 'object' be described as a mere 'aspect' of that which is actual to the attitude maintained in physics? Can volition, which is not a spatial 'thing' nor a physical 'aspect,' be a datum in the most elementary and simple sense of the word?

After so much epistemology, which seemed unavoidable, partly because the support of the tendency in question is drawn from just such considerations, let us turn to the problem of volition already indicated, and consider the points of regard from which volition may be looked upon as constituting lawful material for the psychologist. By volition, one should understand reference to the active or passive aspects of consciousness, in one way or another. Volition technically regarded as a developed complex in experience, rationally expressed, or as the collocation of conscious contents into a definite adjustment at any given moment, or as the psychical correlate of the physical process known as movement, is apart, to a limited extent, from the meaning carried along in this discussion. However, we are not to search for volition in any 'content' as a mere passive possession in consciousness. This caution should not lead us to the length of thinking psychology to be limited to the treatment of bare material of presentation. Relations are valid 'data' for the scientist; and often these are to be found in processes which are natural, recurrent and exhibit sequences.

The data of psychology are the facts in and the facts about consciousness—in so far as the latter may throw any light upon the former—the constant unit of inquiry always being the total state of mind at the given moment. The facts, when specified in the sequence of conscious content, must be treated in their relations to each other, rather than in their relations to brain-processes, or to the objects regarded as stimuli. In general psychology the concrete data do not recur in the time series, since they include the sequences of history regarded totally. Hence the extreme difficulty of establishing psychological laws of historical and social phenomena.

It is only in the analysis of individual consciousness that one can find recurring phases of experience, as they are given, for example, in the life of a plant,—albeit, a mental state when passed is gone forever. The phases are discovered in comparing a given state of consciousness with an earlier or a later state. To secure any sort of a support for a

denial of this labor as a legitimate task for the science of psychology, one must needs go outside the boundaries of that science itself,—a very dangerous proceeding in view of the logical precedence established thereby. In such a manner volition is found to be constantly recurrent in consciousness. A multitude of successive experiences is required before the subject exhausts all possible forms of sensation, movement or feeling. In any instance of the latter, the immediate content of consciousness is not the form of consciousness in general, but the particular modification as sensation, movement or feeling. The volitional reference in any one of these given cases tends to bear an identity with a previous reference, though limited to the narrowest range of qualitative variation.

This definiteness of the volitional process is greatly in its favor towards accepting it as a datum for psychological purposes. Each volition is specific and definite, giving a marked setting to the content of the mental state,—in fact making the state of mind to be what we actually find it to be when described in relation to its object. This feature of volition is not to be identified or confused with the precision characterizing the sensational content of blue, or the ideational content of an image of the moon or of C-natural; but it is none the less pertinent to the analysis of the process, and in so far forces an enlargement of the conception of the content of the science as to admit it on equal grounds with other data.

There is also a certain universality to volition in mental life which entitles it to a more generous recognition than the tendency is inclined to allow. All forms of mind action give evidence of this process prevailing. It may include in its references all types of sensation, ideas, thoughts, all significant movements, and the more important turns which particular feelings may assume. This disposition of volition to unite with the entire variety of content shows strong evidences that volition is as primitive a factor in conscious experience as are those typical receptive modes of consciousness which are traceable to definite stimulations of peripheral origin. Volition is not a 'receptive' process (excepting the mode of attention, which is generically allied to it), but it is to be considered as more definitely connected with the transforming (intellective) and expressional processes of consciousness. That it is not a static content, one can readily agree, without impairing the originality of its character when contrasted with the composition of such content.

Action is regarded as a proper topic for a scientific psychology. The tendency in question obscures that aspect of volition as a process

in consciousness which is revealed in our inability to understand any one of the physical movements the organism may execute, without first knowing something about active consciousness. If a movement is to have 'meaning,' then it stands as the 'expression' of an idea. Even in cases of purely peripherally stimulated movements, they are described and explained as 'non-voluntary.' The concept of volition is necessary, by implication, to characterize such movements. Furthermore, any study of movement as a sequence of definite changes in spatial relations, cannot be fully carried out without implying reference to what the movement accomplishes, either in the objective or in the subjective events. Each movement thus has a real and particular 'function.' It does something, while involving complex structures. This aspect is deeply implicated in the barest conception of the reflex-arc. Reflex movements are those which occur with special reference to a definitely applied stimulus. The movement's function, however, is not to be identified with or paralleled by the function of the ganglionic centers or of the special sensory-motor tracts which may, perchance, be concerned in maintaining the given reflex. The current tendency is too prone to accept movement itself as the primary psychological datum for throwing light upon the nature of volition as a datum secondary thereto. Movement is but a physical phenomenon, and as a mere succession of changes leaves the problem of volitional analysis as dark as ever. The weakness of the position is further shown by the suggestion that the psychologist is more at home, so to speak, both in matters of fact and of theory, when knowing something first about the volitional structure of any given state of mind. This interpretation can be strongly insisted upon in spite of the ordinary procedure of beginning a psychological inquiry of volition with a study of movement. The prime condition of psychological study is not to be found in the presentation of mere movements to one's senses.

There is a certain bit of instructive analogy along this line, even if it is not conclusive. What a movement may be as mere movement is determined by the structure of the organ or organs involved in bringing the movement about, the varying sizes of the muscular fibers, the tendon and joint attachment, etc. These items of structure do not give the slightest clue as to any possible movement the total organ might perform. That is revealed only upon a basis of perceiving the movement in its actual performance. The movement, as such, when occurring, is functional, not structural, and must be considered as the coördination of multiple 'static factors' into the one complex which the movement itself is. The movement is absolutely novel in the his-

tory of the organism, and herein lies its primacy. When development in movement takes place, or even when a repetition of one movement or a succession of several movements occurs, then the student can compare the movements among themselves and get the several empirical facts about the movements as mere occurrences. The point to this analogy is that the tendency under consideration cannot free itself with aid from this side of conscious experience from regarding volition as definite and independent, even in its psychical aspect of mere movement.

When a class of students in psychology is asked to analyze consciousness with the purpose of getting at the volitional 'content,' or the volitional 'process'—as the case may be, for the analyzer is not to be predisposed in his effort—it has been my observation that they generally proceed in a peculiar manner. From the view-point of a teacher my aims are at first well-nigh frustrated by the almost uniform insistence that the reports should be filled in with logically connected ideas *about* volition. The students rather presume that the case is an issue which must be argued out to the end in a truly syllogistic fashion, but always in the interests of some issue other than that contained in the psychological aspects of the problem. (A similar difficulty appears in the first attempts to gain clear ideas about other processes in consciousness, such as sensation, feeling, etc.) Thus, for example, it is said, "freedom is in danger," "the soul as spirit must lead to this or that view," etc. It is made out that one is to choose the starting-point for analysis solely in light of the consequences logically apprehended. I do not believe a teacher has a moment's right to allow such a notion to remain or to prevail in the minds of his pupils. It would mistake analysis of an actual process, and replace it with mere logical dialectic. Psychology must, of course, build up some arguments: but the real problem is, What shall be admitted into those arguments: syllogistic conclusions, or ideas derived from facts as original experiences?

When a tolerably full account is given of the particular percepts, images, or thought-connections, and the momentary feeling-motive which prompts the action actually performed or ideated, there is a uniform confession of 'something more there, though I cannot get at it.' Sensations, images and feelings are thus not looked upon as constituting the whole state of consciousness. Volition is set out in terms of a certain plusness, so to speak; but its factual reference must always be considered in the composition of the total state of mind. The species of particularization is horizon-filling when the enumeration of details of sensations or ideas (and possibly the same is true

of feelings) is carried out with any completeness. In the volitional reference to a conscious state, there can be an enumeration in terms of one kind only, though in the positive or negative directions. This will account in some measure for the disposition of the tendency to regard volition as 'secondary' among the phenomena of consciousness. This 'something more' is given ('felt' as often said); it is recurrent; it possesses a sequence in the specific changes in consciousness or in the physical adjustments of the organs of the body; and the descriptions of it are appreciable at least. Although every analysis makes some immediate reference to the sensory, ideational, and expressional content of consciousness, one is not thereby justified in concluding, with the current tendency, that volition is to be reduced to some one of those elements, or is statable solely in terms of those processes. The position that volition never arises as a 'pure' form of consciousness is all that is guaranteed thereby.

Another mark of the volitional process revealed by analysis is its definite and constant reference to the individual, rather than to the thing or to the 'object' as it stands represented in mind. Persons are found saying "It was, or is, or may be, my volition, or action." Sensations and ideas do not primarily present this aspect, excepting those which the individual attempts to bring into his consciousness. Movements, also, of a voluntary sort, come to be 'my,' 'his,' or 'its' movements. There is no apparent disposition to escape the individual reference when speaking of 'effort,' 'attention,' 'doing,' etc. When the object, on the contrary, by the intensity and quality of its stimulations attacks and holds me, then I am said to be giving, and describe myself as giving 'passive attention.' This is the object's attention. If *I* attack and hold the object, the individual reference returns to the foreground of the experience. Thus all attempts at an analysis of the volitional process forces a constant consideration of the personal side of the data in question as the more important and qualifying aspect. The personality thus conveyed is not the person as the content of a conception of self nor as the substance of which mind may be made.[1]

The foregoing analysis suggests the conclusion that the position which advocates volition as a mere content of consciousness, possess-

[1] A complete analysis of the process would involve a careful separation of all the typical forms of control which experience presents, such as control of retinal images, projected after-images, emotions, ideas in revery and in clear thinking, and of all succession of psychic compounds, as association, fusions, 'synopsie,' trains of images, etc., which may fittingly be reserved for another time. It can then be more clearly shown that the analysis maintained is quite in accordance with the possible development of experimental methods.

ing the varying qualities of sensations or of ideas, is not to be defended. It does maintain volition to be something actually given and ascertainable in experience, which science must recognize in an appropriate manner. The datum which must thus be recognized is a conscious relation, in which any given mental content is placed, adapting it for that expression which marks the completion of all psychological processes. The primacy of volition is not the primacy of sensation nor of feeling. Its simplicity is not such as they may have. It may itself be simple or complex, depending upon the stage and degree of development which the particular state of mind has attained. Complexity, however, is ordinarily a logical relation which is greeted and enforced as the development takes place. In fine, volition is the characteristic, typical, irreducible process into which consciousness places its definite, though varying content.

The tendency to reject the volitional process as original material for the scientific attitude has been seen to be defective when viewed in light of the epistemological considerations and of the results of analysis. There remains a third group of suggestions which have definite force against all justification of the tendency. Let us turn our attention briefly to a few principles of psychological science in so far as it should be obedient to all facts, and as those principles should be recognized in any further treatment of the process. As soon as volition is admitted as a psychological datum of scientific worth, there arises the demand that we shall seek to develop, as the first problem of the science, the inter-relations of the states of consciousness, before going to deal with the darker region of the relation of psychical events to cerebral processes. Whether we consider the mental state in its cognitive relation to its object, or as it is actually discriminable and discriminated by the individual subject of the state, the relation of state to state in consciousness is by far more clear to the student than the second type of relations. Mere mechanical sequence does not prevail exclusively among mental facts, and volition appears as part explanation of this radical difference between the psychical and the physical.

The science of psychology finds another of its special tasks to consist in an explanation of certain typical and rather uniform developments in consciousness. Mere discovery of sensational and ideational elements of experience does not replace this particular problem. There is an integration of the 'elements' of experience, which, as a fact of importance and of actuality, is not transcended by the importance of the actuality of the 'elements' themselves. How shall psychology give a lively and vital explanation or descriptive account of this inte-

gration if all recognition of volition as a datum—which shall be real in that experience and thus explanatory, rather than in need of explanation—is forced out? Integration may, indeed, be looked upon as a function, and the volitional process presents the phases which support that function. If we look upon cognition as an 'object' for the psychologist to relate to other objects (as has been set forth frequently in strongly postulated terms), then he is constrained to make explicit reference to volitional processes. Sensation as mere content and movement as mere sequence do not facilitate any explanation which is able to avoid logical absurdity. To know is to do something in a characteristic manner. And it is barely possible that the root process involved is supplied by the functional activity which volition has been seen to exhibit.

That a rejection of volition as a datum which can be appealed to for aid in really scientific explanations, virtually destroys certain principles of psychology, may be seen in any attempt to understand willing, desiring, wishing, conscious doing, etc. To explain these is a manifest duty of psychology, in so far as they are mental phenomena. That they are complex and developed does not remove the task. That they *are* such phenomena (*i. e.*, real) is attested by the experience we live, the words descriptive of these actualities which have been coined and are appropriated by all our important languages, and the reigning classifications in the science of mind itself. In order to give an explanation of will that shall be logically acceptable and patterned after the 'scientific' explanations given of ideas, or percepts, etc., one must make explicit reference to some simpler process which shall be like will in kind, but primitive in origin, and may mix with other elements of consciousness in the formation of the will compound.

Any consideration of the relation of psychology, as a science of description and explanation, to philosophical subjects shows a decided weakness lurking in the attitude which insists that volition is not to be admitted as a valid datum of the science. One cannot erect a perpendicular line of demarcation among the data of mental experience, and say that the content side belongs to psychology, while the process side belongs to philosophy. If psychology has any relation to philosophy whatsoever, then the total 'sides' of consciousness regarded scientifically must be set off to the credit of the metaphysical and epistemological considerations. To say that sensation has nothing for metaphysics because its content can be specified as visual, or auditory, or cognitive, is a defective analysis to say the least. Will and sensation are alike given in the one stream of consciousness as presented under the con-

ditions of experience. Psychology has the right to interpose an objection wherever favoritism is indulged in when assigning 'values' to the several items it may recognize. From this point of view, volition is really a datum of the world, and science and philosophy are in duty bound to turn their analytical attention to it. In other words, volition is not merely 'a point of regard' in each instance.

In conclusion, it is believed to have been shown that a dangerous tendency is at work undermining the structure of psychology. This tendency does not stand the test of the logical scheme into which scientific activity and its data should be placed. Volition, as analyzed, presents marks which entitle it to be placed among 'primitive' processes in consciousness. As all other forms of mental experience, it never stands alone, but is in constant relation from the beginning with all contents and all other functions of consciousness. The hope that all mental facts can be reduced to a few elementary and thus explanatory processes or forms, is an illusive hope, since volition cannot be 'explained' by sole reference to sensation and affective aspects thereof. Volition is ready and able to explain in a psychological manner, rather than being in need of explanation itself. To eliminate volition as a scientific datum, would be a virtual subversion of the science of psychology so long as it is developed with the intent of representing the nature of mental activity and the course of its development in terms of a real and acceptable knowledge of facts.

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VISION WITH THE MOVING EYE.

In view of Professor Dodge's experiments, published in the present number of the REVIEW, I am uncertain as to the validity of the hypothesis advanced by me to explain the lack of fusion in vision with the moving eye.[1] I had planned similar experiments, but was unable to make them, although I held my paper for a year,[2] in the hope of being able to do so. Having made the observation that there is no fusion in the vision of daily life when the eye is in constant motion, I attempted to explain the fact by assuming that the visual organs respond to retinal changes more rapidly when the eye moves than when the objects are in motion. Professor Dodge finds that there is fusion

[1] This REVIEW, 7: 325–343, July, 1900.

[2] It was presented to the Physical Section of the American Association for the Advancement of Science in August, 1899.

when objects are exposed only when the eye is moving. It would follow from this that the hypothesis discussed in my paper, but there rejected, must be accepted or at least considered. This is, that there is clear vision only in the pauses of movement, there being no peripheral perceptions while the eye is moving and the apparent distinctness of the field being an illusion. This hypothesis will not interfere with the chief conclusion of my paper, for, as I said, "if one had in fact only a few distinct images in the course of the movement, it would remain true that a series of successive images is seen as a space continuum." Indeed, if it turns out to be true that we have no perceptions corresponding to the retinal changes while the eye is moving, but supply centrally the perception that we should have if the eye were at rest, this would further support my argument for the dependence of perceptions on utility and experience rather than on the nature of the physical stimuli.

J. McKeen Cattell.

PSYCHOLOGICAL LITERATURE.

The Psychology of Religion: An Empirical Study of the Growth of Religious Consciousness. EDWIN DILLER STARBUCK. London, Contemporary Science Series, WALTER SCOTT. 1899. Pp. 423; with Preface by WILLIAM JAMES.

Professor Starbuck's book is in substance the reproduction of two long papers published in the *American Journal of Psychology* (Vol. VIII., 1897). It is the most elaborate research yet published in a field of investigation made fertile by the use of methods which, until recently, had been unjustifiably reserved to other fields. The purpose of the book and the method employed are described in the Introduction: "It is a purely empirical study into the *line of growth in religion* in individuals, and an inquiry into the causes and conditions which determine it. It begins with conversion, since that seems to show in a condensed form some of the essential features of religious development. Then follows a discussion of the line of growth following conversion. Part II. is a presentation of the line of growth where not marked by apparently sudden changes of character. The material for the study consists largely of autobiographies written in response to a printed list of questions. The task before us is to take the varying records and find what are the common elements in them; to get a composite picture of them; to discover what are the larger aspects of religious evolution in masses of people, and to approach an insight into those laws of growth which, for the groups studied, seem natural and normal." This proposed task is so gigantic that I suspect many psychologists and the majority of the persons interested in religious studies to whose notice the book may have come have looked it over primarily to see how radically the author had failed in his foolhardy undertaking. But those who have had the patience to read him through carefully have, no doubt, closed the book, not indeed with a sense that the task had been completed for good and all, but at least with the conviction that it had been begun in good earnest and with a considerable share of success.[1] They will, moreover, have been con-

[1] In April, 1896, nearly a year before the first paper of Professor Starbuck, I published in the *American Journal of Psychology* (Vol. VII., pp. 309-385) an extensive study on conversion; it covers much of the ground traversed in the

vinced that there is here, in the patient gathering and careful consideration of the subjective facts of religious life, the foundation of a new body of knowledge which will find its place in psychological science and bear practical fruit in religious education and in theology.

That Professor Starbuck's book is open to almost numberless criticisms of detail and to a few of fundamental importance is to be expected when one considers the difficulties inherent in the subject and the too ambitious attempt made to touch upon almost every part of it. But, whatever legitimate criticism may be offered and however easy it will be for academic drones and effete theologians to hold it up to ridicule, a debt of profound gratitude is due the author for his persevering and earnest effort in gathering and interpreting a large mass of information on individual religious life. It is so much more comfortable to let loose one's constructive imagination and to spin out diverting speculations on a dubious basis of facts selected from the labors of some one else!

By far the most valuable part of the book is the first—'Conversion'—filling about half the volume. The second and the third parts —"Lines of Religious Growth not involving Conversion" and "Comparison of the Lines of Growth with and without Conversion"—offer much less interest. We shall therefore devote our analysis chiefly to the first part. Lack of space will prevent us from entering upon the discussion of the opinions and conclusions reached by the author as it would carry us beyond the space at our disposal. A discussion of several of the more important topics dealt with in this book will be found in the reviewer's paper above mentioned. In closing we shall offer a few criticism on points of general bearing.

Chap. II. The Sources.—The material on which the first part (pp. 21–183) is based consists of 192 cases of conversion, nearly all obtained in answer to a questionnaire too long to be reproduced here. They are nearly all from Protestants; the Methodists predominate. The term *Conversion* is used in a very elastic way: "The more quiet and unemotional experiences were taken along with the sudden and violent. The only test was that the person believed that the event represented a real turning-point or the beginning of a new life" (p. 24).

Chap. III. The Age of Conversion.—For the making of the

first half of the book before us. The method is the one which Professor Starbuck has used, minus the statistical tables. The third and concluding part has remained so far unfinished. To prevent misapprehensions, I desire to say that Professor Starbuck had conceived the first idea of his research and had begun to circulate one of his questionnaires before my paper appeared in print.

charts and tables of this chapter, the age of 776 converts, found in the Alumni record of the Drew Theological Seminary (Methodist), was added to the data collected by the author. The age-curves present three summits which are interpreted as follows: "We may safely lay it down as a law, then, that among the females there are two tidal waves of religious awakening at about 13 and 16, followed by a less significant period at 18; while among the males the great wave is at about 16, preceded by a wavelet at 12, and followed by a surging up at 18 or 19." The conclusions are reached that "the first two rises in the curves for conversion seem then, from the psychological standpoint, to correspond to the decline of the sensory elements in consciousness, and the birth of rational insight," and "conversion and puberty tend to supplement each other in time rather than to coincide; but they may, nevertheless, be mutually conditioned." Conversion, as the author understands it, is a distinctly adolescent phenomenon and is connected with the physiological growth of that period of life.

Chap. IV. The Motives and Forces leading to Conversion.—An insignificant chapter on a most important subject; eight pages with two statistical tables showing one 'the relative frequency of certain motives and force which lead to conversion,' the other 'a comparison of the revival and non-revival cases in regard to the motives and forces leading to conversion,' and curves 'showing the frequency of various motives for different years.' We reproduce the first table (p. 52).

Motives and forces present at conversion.	Females.	Males.	Both females and males.
	Per cent.	Per cent.	Per cent.
1. Fear of death or hell	14	14	14
2. Other self-regarding motives	5	7	6
3. Altruistic motives	6	4	5
4. Following out a moral ideal	15	20	17
5. Remorse, conviction for sin, etc.,	15	18	16
6. Response to teaching	11	8	10
7. Example, imitation, etc.,	14	12	13
8. Social pressure, urging, etc.,	20	17	19
Sum of 1 and 2—*Self-regarding* motives	19	21	20
Sum of 3 and 4—Other-regarding and ideal motives	21	24	22
Sum of 1 to 5—Subjective forces	55	63	58
Sum of 6 to 8—Objective forces	45	37	42

From this table the two following propositions are derived, rational considerations play a small part in conversion; "males are controlled more from within, while the females are controlled more from without."

The three following chapters (Experiences preceding conversion; The mental and bodily affections immediately accompanying conversion; In what conversion consists), always in statistical form, prepare the way for Chap. VIII., the pivotal chapter of the first part. Here is one of the several tables found in these chapters; it shows 'the relative prominence of the various mental and bodily affections for both sexes.'

Mental and bodily affections.	Female.	Male.	Total.
Sense of sin	33	33	33
Feeling of estrangement from God	24	24	24
Desire for better life	18	18	18
Depression, sadness, pensiveness	75	60	70
Restlessness, anxiety, uncertainty	45	33	41
Helplessness, humility	12	9	11
Earnestness, seriousness	12	1	8
Prayer, calling on God	39	57	45
Tendency to resist conviction	36	50	41
Doubts, questionings	6	15	9
Loss of sleep or appetite	26	29	27
Nervousness	6	5	6
Weeping	9	3	7
Affection of sight	5	6	5
Affection of hearing	3	7	4
Affection of touch	14	12	13
Average duration of conviction	24 weeks.	69 weeks.	

Here again, according to the author, we see that "conversion is a process in which the deeper instinctive life most strongly functions." This is no doubt true, but I am at a loss to see that these statistics bear that interpretation. Much space and figuring are devoted in these three chapters, and also throughout the book, to a comparison of the sexes with each other, and of conversion as it takes place in revival meetings and out of it. Here are samples of the propositions drawn from these comparisons: "feeling plays a larger part in the religious life of females, while males are controlled more by intellection and volition;" "the males when not at revival insist more upon seeing their way clearly toward the new life, they are more wilful in the stress of a revival, and the revival conversion is a far more intense experience."

"The sense of sin and the accompanying depression of feeling are fundamental factors in conversion if not in religious experience in general. We must not look upon the sense of sin as simply a spiritual fact, but we have to look for the cause underlying the sense of sin, in part, in certain temperamental and organic conditions." (Pp. 67 and 71.) As a conclusion of the preceding analysis the typical conversion

experience is described in Chap. VI. in a few barren words, "The typical experience has three distinct features, viz., dejection and sadness, a point of transition, and lastly, joy and peace." At this point the reader should have before him a clear mental picture of the conversion experience, but it can hardly be so as not one whole case has been given him; instead isolated sentences from the respondents, now on 'sin,' now on 'estrangement with God,' now on 'love for others' or some other part of the experience fill the volume. It is as if instead of seeing a few typical heads one was shown successively dozens of noses, of upper lips, of under lips, of chins, etc. What is the meaning of an eye seen apart from the rest of the face? The reader, unless he be intimately familiar with the experience dissected, will have the impression of moving in a maze of disjointed facts and much of the meaning of the book will escape him.

Professor Starbuck is aware of the large place that must be accorded to unconscious cerebration in the dynamics of mental life in general and particularly of conversion, and he has noticed that self-surrender, self-renunciation, is very frequently an apparently necessary condition of it. Hence, when he comes in Chap. VII. and VIII. to close quarters with the causal determination of the conversion crisis, he finds it necessary to consider at some length the rôle of the will and of consciousness. In Chap. VIII. the problem is formulated thus: "The question is not simply how much of conversion is willed, but how much of the process as it is being wrought out rises into consciousness; and on the other hand, is there evidence that part of the process is worked out automatically by the nervous system?" Arguments and facts have already been given to show that a part of the phenomena preceding conversion are instinctive. It should not be thought that when at the decisive moment of conversion the will of the individual is passive it is therefore not concerned in the process at all. On the contrary, the final victory, the new birth, is in a large measure the result of previous acts of the will striving towards righteousness. (Pp. 97–100.) How this is possible the author endeavors to make clear to the lay reader by a digression on the effects of past unconscious upon present conscious activity, and of past conscious upon present automatic activity. In the three closing pages of Chap. VIII. an effort is made to grapple with some of the most intensely interesting and fundamental psychological questions raised by the conversion experience as the passivity of the will, faith, and the nature of the new life. I can do no more than quote the italicized passages: "Self-surrender, then, is often necessary in order

that the normal tendencies of growth may converge and flow into harmony, and that the point of new insight may be, for the person yielding, the truest organizing center of life." "The act of yielding, in this point of view, is giving oneself over to the new life, making it the centre of a new personality, and living, from within, the truth of it which had before been viewed objectively."

Chap. IX. The Quality of Feeling following Conversion.—Joy and peace now follow the anxiety and despondency of the pre-conversion period. A common experience with new converts is the *sense of newness*. Two statistical tables are given. The author looks upon these feelings as the token of 'the functioning of a new and exalted personality.' This view of conversion is developed in Chap. X. and particularly in Chap. XII. At conversion, life is organized around a new center, around impulses which may be characterized roughly as altruistic—it is an 'unselfing'—to contrast them with the egoistic motives forming the center of the old personality, hence the larger life of the subject and the birth of new powers. In Chap. XII. ('A general view of conversion') the new life is looked upon from the physiological and from the psychological point of view as the formation of a new person. Reference is made to the growth of the brain fibers, their medulation, association, etc., and thus some plausibility and definiteness are given to neural changes supposed to take place simultaneously with the formation of the new *ego*.

Some valuable light is thrown in this chapter upon the problem in discussion.

Chap. XI. ('Conversion as a normal human experience') gives many instructive instances of depression and joy, of sudden awakenings, of sudden changes in emotional attitude and of breaking of habits, occurring independently of religious influences. These illustrations were obtained in answer to a special set of questions; they are given to add weight to the presumption that conversion can all be explained in terms of natural psycho-physiological processes.

A chapter on the abnormal aspect of conversion concludes the first part of the book.

The data used in the second part (pp. 183–350) consist chiefly of 237 answers to questions on religious growth designed for persons who are strangers to the conversion—experience. It includes chapters on the religion of childhood, on various phases of adolescence (six chapters; religious awakening, storm and stress, alienation, etc.), on the period of reconstruction following adolescence and finally three chapters on adult life. The chapters on adolescence deserve especial attention.

The third part (pp. 353–420) is an attempt to trace out the chief lines in the growth of religion from childhood to maturity and to compare growth via conversion with growth not including it.

I have, I trust, sufficiently expressed my appreciation of the solid worth of Professor Starbuck's work to venture the following general criticism without running the risk of unduly depreciating it in the eyes of the readers of this REVIEW.

I have already deplored the absence of complete religious biographies. At least one of each of the several varieties of experiences gathered by the author under the name conversion should be found entire in the book.

One of the serious weaknesses of the book is the totally insufficient use made of introspective analysis. To yield the best result, introspective analysis, and the kind of interpretation and of explanation which it makes possible, should have gone side by side with the objective consideration and the statistical tabulation of the data. But the author has chosen to remain outside his facts, as if he was handling physical substances. Throughout the volume we deal—we might say *figure*—with *things* called sense of sin, remorse, example, response to teaching, depression, feeling of estrangement, tendency to resist conviction, sense of oneness, divine aid, etc., etc., and no effort is made to analyze these things of which the respondents speak and to translate them in psychological language. The *words* in circulation in religious circles are too often the coins with which the business of this book is transacted. Phrases like the following make one balk when met in the writings of a professional psychologist: 'A vital experience of spiritual truth,' 'the attainment of spiritual life,' 'the person has acquired a spiritual grasp, a new insight,' 'the higher life of intelligence and insight,' 'a personal hold on virtue,' 'a first hand perception of right and wrong.' An investigation exclusively from the objective standpoint, the only one available to the physical scientist, is self-evidently doomed to remain on a very low explanatory level when its objects are processes of immediate experience.

What shall be said of the statistical method which gives to this book a unique appearance of scientific rigor? The propriety and the advantage of using statistically the material obtained by means of syllabi on subjects as complex as religious life is open to weighty objections, the chief of which arise, in the case before us, from the loose meaning ascribed to the term conversion, from the unavoidably confused and inexact language of the correspondents describing their experiences and from the comparatively small number of answers tabulated.

It is evident from the citations scattered through the book that a definition of conversion, as the one we have quoted at the beginning of this review, tends to bring together experiences differing greatly from each other in quality as well as in intensity. In some instances a mature person relates an experience covering years of moral struggle and reaching down to the springs of life; it culminates in a crisis from which the subject issues a new man. In other cases what is described are the skin-deep, momentary, feelings of a sentimental adolescent female who, in the excitement of a revival meeting, goes to the altar 'to give her heart to Christ'—an experience which hardly leaves any trace behind. The study and the mutual comparison of these and other 'conversion' experiences is evidently a legitimate task, but to put them together in order to treat them statistically comes dangerously near figuring without paying attention to the place of the digits. The author speaks of obtaining in this way a 'composite picture.' What kind of composite picture would be obtained from faces in which the distance between the eyes varied from 1 to 2 inches? An elongated grayish spot twice the length of an average eye and increasing in darkness toward the center would be the composite eye; a most instructive blur! We cannot enter into a detailed examination of the effect of this procedure; the whole book is more or less vitiated by it; we must be content with an illustration involving the chapter on the age of conversion. The average age of conversion for male is given as 16.4 years and that of its most frequent occurrence as 17. The average age of the conversions I have gathered for my printed paper is around 25 and the year of its greatest frequency is not far from the same figure, this simply because my questionnaire was understood to call for descriptions of conversions of the deep, life-transforming variety; the variety that can occur only when the great plasticity of early youth has been superseded by a character more firmly knit. What becomes then of the ingenious and interesting generalizations of Professor Starbuck putting in relation the age of conversion with that of puberty and of greatest bodily growth? They hold exclusively for one variety of conversion, a variety to which a large number of the conclusions reached in the subsequent chapters do not apply. The conditions imposed by the statistical method have not been fulfilled in this book, therefore the statistics, as far as they lump together experiences which should have been held separate, produce but a spurious precision and often introduce confusion and error instead of distinctness and light.

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The Making of Character—Some educational aspects of ethics. By JOHN MACCUNN, M.A., LL.D., Balliol College, Oxford. New York, The Macmillan Co. 1900.

The person who takes a well-worn and hackneyed subject and by a fresh and vigorous treatment imparts to it a new and facinating interest, has performed a real service for his kind. The more fundamental the subject the greater the service.

Innumerable books have been written on 'character.' The subject has been talked about and preached about from the beginning to the present time, and it would seem as though the last word had been spoken, but here we have an author who ventures to risk his reputation as teacher and scholar in writing a book on this venerable topic. That he has abundantly succeeded, no one will doubt who, possessing a liking for ethical studies, will peruse the work.

The point of view is that of the educator, and the current dictum that the highest aim in education is the formation of character, is not overlooked for an instant. The table of contents indicates that all the forces, natural and artificial, which operate to produce the man or the woman, are taken into account. Under the head of 'Congenital Endowment,' Heredity, Vital Energy, Temperament, Instincts, Development and Repression, Habit and its Limitations, are all carefully considered.

The author never fails to seek a just balance between what is natural and what is artificial in life. He says: "It is an impossible task to discriminate sharply between what is congenital and what is due to the influence of environment. Environment begins to operate with the beginnings of life, nor does it ever cease to operate, not for an instant, as the days become months and the months years."

His view of what temperament is reflects the same broad and comprehensive method. "Of this mixing of the elements, temperament is the reflection inwrought in the very texture of the life that modifies all we receive and from first to last conditions all we do. It is, so to say, the medium that colors and suffuses all experience."

Under the general caption of 'Educative Influences' the book deals fully and specifically with bodily health, reactions, family, school, friendship, livelihood, citizenship, and other social influences.

Professor MacCunn finds a close relationship between the physical and the moral. "Idleness, says proverbial wisdom, comes to want, but its worst want is not the empty purse; it is the soul atrophied for lack of the spiritual wages that never fail the strenuous life. What holds of idleness holds likewise of languor and weakness."

The chapter relating to the Wordsworthian education of nature is particularly suggestive. "Exclusive preoccupation with society is not the way to know it best. Through communion with nature man may come to understand life better."

While great stress is laid upon the physical basis of life and growth, the author does not overlook the importance of moral ideals and especially of those ideals which dominate social activity. He finds that not only in the personal life but in all organized effort there are guiding principles which inspire the human heart. "It is precisely in the sphere of industrial and commercial life that ideals are most needed to uplift the practice of the world."

This profound study of social and ethical values finds its climax in the two concluding chapters on Self-Development and Self-Control. Here we have the latest and best educational thought of our time. No word has been spoken which better voices the deep convictions of parents and teachers to-day than the idea that all growth and training must seek to develop a self-poised, self-governing being who finds his freedom in the proper use of all his powers and who experiences his highest joy in serving his fellow-men.

SAMUEL T. DUTTON.

COLOR VISION.

Ueber relativen absoluten Mangel des Farbensinnes. E. RAEHLMANN. Ztch. für Angenheilkunde, II., 315, 403. 1899.

Raehlmann adds some observations on partial color-blindness, but by far the most important part of his two articles is that which gives a detailed account of a very remarkable case of total color-blindness, the only one hitherto discovered in which the eyes of the patient were in every other respect perfectly normal. No trace of disease or of abnormity could be detected except the lack of color-vision: there was perfectly normal visual acuity and rather more than that, normal 'light-sense,' and normal visual field. The distribution of brightness in the spectrum, however, resembled that of no other case of either disease or health; there was an extreme darkening in the part which is yellow to normal eyes, so much so as to divide the spectrum almost into two separate regions; the normally blue part, on the other hand, was distinctly intensified. Raehlmann did not apparently make use of a spectrophotometer, but there seems to be no doubt that the patient was extremely insensitive to, Raehlmann says, yellow light—it would be better to say light of the yellow wave-length, as yellow

light did not exist for her. Here as in other respects, language needs some remodeling in the interest of more accurate distinction between the objective and the subjective. The patient, an intelligent lady and the wife of a university professor, had unusual powers of discrimination in regard to her sensations, and unusually good capacity for observation. There are several cases of color-disturbance in her family, and especially that of a son of the Frau Professor herself; his vision is dichromatic, but he has very keen sense of difference of brightness. Mrs. R. is wholly lacking in any idea of what a color is. It is said that an oil-painting does not correctly represent to her the relative brightnesses of a landscape, which are better given in a photograph, and that a picture gallery seems to her an absurdity. The spectrum is increased in length at both ends, more especially at the violet end; it is brightest in the green—there is also a maximum in blue and again in red. Adaptation to a faint light was normal. The threshold in the yellow part of the spectrum was extremely high, but in the blue it was far lower than for the normal eye. Visual acuity, which was normal at an ordinary illumination, was in an extremely faint light twice as good as that of any of the five assistants of Professor Raehlmann who examined her. After-images were normal, but simultaneous light induction seemed not to take place (p. 331). There was astonishingly acute perception of brightness differences, even at a strong illumination, and there was no trace of photophobia.

This case of Mrs. R., while it is absolutely unique, attaches itself readily to the cases of dichromasy with perfectly undisturbed vision in other respects, which are no rarity. It is evidently to be referred to some defect of development in the visual centers, and is to be connected with those cases of achromasy in which (although vision is markedly defective) there is no foveal blindness, and hence there is no reason to believe that the entire cone-system is out of function. That the brightness curve is totally different from that of nearly all the other achromates and also, consequently, from that of the normal eye in the reinforced vision of darkness, is by far the most remarkable phenomenon presented by the case, and it must stand for the present as something totally disconnected with all the other facts of vision. So far as it goes, it gives substantiality to the idea of an abnormal shift of excitability (not, indeed, in the retinal visual substance but in the cortical receiving stations) of which the Helmholtz school makes so much in its explanations of defects in color vision. But even before this the shift of excitability hypothesis could not be said to be an instance of *non vera causa*, for already there were known the cases of what are

called anomalous color vision in which the maximum red sensation is produced by a light wave of not the same frequency as in the normal eye. These cases, too, it is not the custom to keep sufficiently in mind.

Raehlmann speaks of this insensitiveness of the eyes of Mrs. R. in the region of the sodium line as if it were a sort of yellow blindness over and above the absence of specific color sensation throughout the spectrum; and he even says (p. 334) that in a sense every gray-sensation contains a color component (because it is produced by ether rays, which, if they acted alone, would give rise to a color-sensation). But this is visionary in the extreme.[1] It is the great advance of recent years in this field to have shown that the quenching of opposite colors is an affair of physiology quite below the level of consciousness and that the colors which are mutually destroyed have never had an existence as colors. Here again appears the need of a better phraseology —of distinguishing between the spectral *colors* and the regions in the gamut of ether-vibrations which give rise to them.

This case ought also to have the effect of causing us to accentuate more than we do now the difference in nervous level at which color-defect may take place. The central color scotoma of the tobacco and alcohol amblyopia is surely due to disease of particular bundles of the optic nerve trunk—a sort of fact that the psychologist is too little apt to have in mind; in general not enough attention has been given, in the discussion of color-blindness especially, to such pathological cases as are plainly due to retrobulbar lesions—there has been too constant an attempt to explain everything by abnormal processes in the retina. On the other hand, the authenticated cases of uniocular color-blindness can be explained only by an ante-commissural defect; it is, therefore, necessary to bear well in mind that different cases of defective color-sense may be of widely different origin.

Raehlmann calls attention to a very important experiment made by himself twenty-five years ago (Arch. f. Ophth., XIX.) in which a dichromate was given santonine with the effect that his yellow-blue vision was reduced to vision of a single quality only, blue; the whole warm end of the spectrum became to him gray. Such a case as this speaks strongly (and there are other indications leading in the same direction—cortical lesions, chromatopsias, etc.) in favor of their being (whatever the number of chemical processes in the retina) four distinct centers in the cortex for the four fundamental hues. Knies

[1] Moreover, he quite forgets that in night-vision, and in the achromatic vision of the extreme periphery of the eye, there is no question of color-components (in the subjective sense) being by any possibility present.

seems to have shown (Arch. f. Augenh., 37, p. 102) that in santonine color effects the source of the change is not any coloring of the retinal layers of the eye—that is, he draws this conclusion from the fact that in dogs and frogs, after very strong doses of santonine, the visual red, after darkness-exposure, had its color not in the least disguised.

C. LADD FRANKLIN.

The Order of Development of Color Perception and of Color Preference in the Child. By WARD A. HOLDEN, M.D., and K. K. BOSSE, B.S., New York City. Archives of Ophthalmology, Vol. XXIX., No. 3. 1900.

In this paper are described the results of a series of experiments made on over 200 children between the ages of six months and thirteen years, relative to some of the phases of developing color perception. In the first part of the paper, by a method devised by Dr. Holden, the child is tested for its absolute color perception by making use of a background shaded from dark to light gray upon which squares of colored tissue paper are so placed that their luminosity is equal to that of the gray background. The child that reaches for a colored square under these conditions, must perceive that color as differing from gray. At the age of six or seven months infants react readily to red, orange and yellow but without much certainty to green, blue and violet. At eight or nine months they react promptly to the first three, and sluggishly to the last three. At ten or twelve months there is often equally prompt reaction to all colors.

Preference tests carried out with satin ribbons showed that children below three years preferred the red end of the spectrum, selecting red first and then following very closely the order of the spectrum, and selecting blue or violet last. After the end of the third year the order of selection is reversed, however, blue or violet being chosen first and red last. This remarkable change is accompanied by a general disinclination on the part of the child to make any selection at all, so that at the age of about three years only a small percentage could be induced to perform the required action. The results thus obtained were tabulated and the figures give a fair picture of the gradually changing conditions. As a brief summary: The child passes through these phases of development: Up to about the third year the red end of the spectrum is preferred, then follows a period in which the attractiveness of the blue end increases until it becomes as strong as that of the red end, the consequence being a disinclination to make any

selection at all. This period may vary greatly in duration. Later the attractiveness of the blue end increases with the result described. The table in the original shows also that, while at the beginning of the preference for blue the reverse order of the spectrum is followed closely, with advancing age this order changes gradually and approaches the order of preference found in adults. Thus the young infant reacts in a physiological way to the more exciting colors of the red end of the spectrum. The older, mentally advanced child rejects the exciting reds and oranges and prefers the more restful colors of the blue end of the spectrum. These results may lead to further investigations. The described changes of preference in a child of about three years must certainly be parallel to alterations in the mental make-up. If we consider that at this age the reflective stage has begun to take the place of the earlier impulsive stage, the results should yield valuable data as to the character of some of the mental changes in the child. It is likely that an investigation into the development of the other senses would give results analogous to these.

K. K. Bosse.

MEMORY.

Experimentelle Beiträge zur Lehre vom ökonomischen Lernen. Lottie Steffens. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. Band 22, Heft 5. Pp. 321–382.

This paper is chiefly devoted to a report of a series of experiments dealing with the methods by which various persons memorize verse. Having discovered a number of such methods, the author undertakes to determine which form of procedure requires the least expenditure of time in order to learn by heart any given selection of poetry. Throughout the article the term economy applies to the amount of time, and not to the amount of effort, involved in any of the various undertakings. The larger part of the experiments were made upon seven subjects. This is relatively a small number, but the results are sufficiently uniform to warrant considerable confidence that they represent typical tendencies at least. At a number of points the evidence was supplemented and confirmed by experiments with nonsense syllables.

Apparently the natural disposition of most persons, when attempting to memorize verse, is to split the selection up into a number of sections, which are learned more or less independently, and then put together. This general method shows many individual modifications, both as regards the length of such sections, and as regards the relative

number of repetitions accorded the several parts after they have been once learned. Very rarely is there any tendency manifested to learn an extended piece of verse as a whole, *i. e.*, to read it entirely through without the repetition of words, lines or sections. Unusual as this procedure seems to be under ordinary conditions, the experiments show uniformly better results when this method is adopted.

The author advances a number of plausible explanations for the common prejudices in favor of the piecemeal mode of memorizing and then attempts to prove in what particulars the other method is superior, calling to her assistance for this purpose nonsense syllables. The method of learning the selection as a whole has the obvious advantage of emphasizing the really helpful links of connection between the various parts, instead of the fictitious and misleading ones, which often arise when the material is learned in sections. Involved in this is the helpful influence of associations due to the absolute position of given elements in the whole. The importance of this factor will be readily understood by those who have experimented at all extensively with the memory processes. On the other hand, it is to be said that when the material to be memorized is not reasonably homogeneous, the segmental method may be more advantageous. The reason for this is obvious. A peculiarly baffling group of words may render many repetitions necessary, and where the whole selection is read through consecutively, the loss of time is inevitable. With homogeneous material, however, the advantage of this method is roughly proportional to the length of the selection. There is seemingly little tendency, when this method is employed, to rely upon purely mechanical motor processes, by means of which children often commit quantities of poetry to memory without the slightest suspicion of the meaning. Clearly the value of logical significance is at a maximum with the method. It promises, with material of uniform difficulty, the least amount of unnecessary and superfluous repetition of any portion.

Incidentally, in the course of the paper, a number of interesting points are touched upon, such as the relation of rapidity of reading to rapidity of learning; the dominance of sensory or motor elements in the different modes of memorizing, etc.

A final section discusses a series of experiments dealing with the question of the most advantageous method of distributing any given number of repetitions throughout a constant time. Here again nonsense syllables furnish the material. The general results indicate that the distribution of repetitions which affords the best results is that which secures the maximum number of *groups* of repetitions, with

intervals of time between the groups, and the minimum number of repetitions in each group. This means doing a little at a time and doing it very often. Like the conclusions of the first part of the paper, this runs counter to common prejudice and practice.

The author makes no attempt to discuss the question of permanence of retention, as connected with her problem of expeditiousness in learning, nor is there any notice of the modifications in her results, possibly due to the metrical nature of her material. This is not to be interpreted as a criticism of the work, which seems to have been intelligently planned and carefully executed, but simply as an indication of its limitations.

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PERCEPTION AND RETENTION.

Zur Untersuchung der Auffassungsfähigkeit und Merkfähigkeit. JACOPO FINZI. Psychologische Arbeiten (Kraepelin). Bd. III., 2 Heft, 1900. Pp. 289–384.

Finzi's investigation is a methodological inquiry in which he undertakes a more exact examination than has heretofore been made of the disturbances of memory in the normal individual, for the purpose of applying the method in the diagnosis of mental diseases. It is impossible to determine the quantity of impressions which his past life has furnished to the patient, but it may be useful for the alienist to ascertain his capacity for receiving and retaining impressions at present.

The writer seeks to measure two things: the clearness of the impression and its fixity. In connection with the latter aspect we must separate the effects of fixation through voluntary repetition, and the involuntary retention of impressions which follow directly from their presentation. For his material the writer turns to visual impressions, and adopts the customary letters, syllables and pictures. The mechanical conditions of experimentation seem to have been carefully looked after and the apparatus for control of the exposure is fully described and pictured. To secure constancy a Welsbach lamp was used as the source of illumination and the figures were seen by transmitted light. The time of exposure for each point of the surface was approximately the threshold for the perception of letters, viz., 16.7σ. The action of the apparatus was under control of the observer himself. Five sets of experiments were carried out. In the first the interval between presentation and recall varied, the periods being 2, 4, 8, 15 and 30 secs. In the second a constant interval was filled by such distracting media

as counting, adding and reading. In the third the number of stimuli was varied, 4, 6 and 9 letters being used. In the fourth the stimulus was reinforced in various ways—such as reading aloud or writing down—or was not expressly reinforced in any way. In the last set different kinds of stimuli were obtained by the use of letters, numbers and syllables. Only the first of this series, it must be noted, was carried out on a sufficient number of persons to give a basis for more than individual results.

Two factors which appear in the results are discriminated in the writer's analysis. These are the total number of elements perceived or retained, and the number correctly apprehended or recalled. The former is taken as an index of the scope, the latter of the reliability, of the process involved.

The character of the results depends upon a group of conditions which includes both the properties of the material and the mental activities of the subject himself. Among the former are to be reckoned the comparative legibility of the letters and figures, the number of them and of their possible combinations, habitual verbal association, and the like. On the part of the subject the prevalent concentration of attention upon the first cross row of elements—which were presented in three horizontal rows of three—led to the fixation of particular groups of letters and the overlooking of others: the images of one card's contents were in many cases influenced by confusion with letters from preceding cards; and various subjects showed preference for particular letters.

The increase in the number of elements presented has an unfavorable effect upon the scope of apprehension, marked by increase in the number of misplacements; for the retention of the impression it has little significance.

In neither scope nor reliability is the maximum effectiveness of the perception process exhibited immediately after presentation. The former phase attains its highest value between eight and thirty seconds after exposure, the latter at an earlier period, between four and fifteen seconds. Thereafter in both directions a falling off appears. The increase of error may continue, as is to be expected, for a much longer period.

The prevailing forms of error are misplacements, substitution of resembling letters, and those arising from preference for particular letters and the influence of preceding letter-groups. Numbers are better apprehended than letters, but not so well retained. Perception is aided by the fewness in number of the digits as compared with that

of letters, while the greater retentiveness for letters is probably to be referred to verbal associations and grouping. This factor appears again in the statement that letters are better apprehended and retained when presented as parts of nonsense syllables than when seen singly.

The mechanism of retention appeared to affect the process of revival, dependence upon images of the material presented leading to better results than such as involve the use of images of speech.

The effect of distraction during the interval between presentation and recall appears in a reduction of the correct responses while the number of incorrect reports may be either increased or diminished. The latter must be regarded as an individual variable, since while in one subject the fading out of the impression would be marked by an increase in the number of incorrect reports, the total number of impressions reported remaining unaffected, in one of less confident nature those cases upon which uncertainty lay would not be reported at all. As the intervening interval increases subjective certainty declines with greater rapidity than the number of correct responses. Practice increases the reliability of the perception or revival process to a greater degree than its scope. The results of the investigations present little that is new in direction, and such elaborately detailed experiments are justified only in so far as their carefully guarded conditions constitute a needful exactitude in the corroboration which they give to the work of previous investigators.

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REACTION-TIME.

Reactionzeiten bei Durchgangsbeobachtungen. NICOLAUS ALECHSIEFF. Philosophische Studien, XVI., Heft I., pp. 1–60, with two charts. May, 1900.

The paper gives a brief history of the personal equation, an analysis of the different types of reaction, and a discussion of the conditions most favorable for transit observations.

Many of the causes of the differences in these observations, first noted by Maskelyne in 1795 and studied later by Bessel (1822), have since been made the subject of experiment and analysis at different observatories. With the old eye-and-ear method the differences were not the same between different observers; in some cases they remained small and constant, in others they changed progressively (Greenwich) or even suddenly—a difference of .22 sec. developing in one night at Altona. Respold (1838) and Arago (1842) introduced the chrono-

graphic method. With this the differences became smaller and more constant. Prazmowski and Hartmann studied the influence of practice and imagined the error of observation might be reduced to zero by preliminary work with a machine that would show the extent of error made. By means of an artificial transit experiment Hirsch and Plantamour found that the time between the perception and the registration varied from .15 to .36 sec. By others, the influence of the size, intensity, and direction of motion of the star, and the position and exhaustion of the observer had been noted; but a number of psychological factors have not been taken into account by astronomers, particularly the effect of directing the attention on the one hand to the star and on the other to the registration, a variation not known even to psychologists till the publication of Lange's paper in the *Studien* in 1888. This gives us the two-fold purpose of the investigation—the analysis of the different types of reaction, both in the usual form of experiment and in transit observations, and secondly thereby the ascertainment of the best conditions, particularly psychological conditions, for such astronomical work.

In the tabulation of results the author makes use of the well-known statistical method of the frequency of cases. The number of reactions falling between successive hundredths of a second are counted and the results, which appear in both tabular and graphic form, give a better picture of the characteristic variations than could be obtained by the customary average and mean variation. In fact the records in a series by the same person are sometimes so distributed that the arithmetical average falls between two points of high frequency rather than at the point of greatest frequency. This is particularly so in the 'natural' reactions in transit observations—only artificial transit observations are made throughout in which a tendency to employ now a long and now a short form of reaction in the same series was observable.

The sensory and motor or, as the author very properly prefers to call them, the more and less complete, or the longer and shorter, forms of reaction are to be looked upon as extreme variations from the 'natural' type, or that which the reactor adopts spontaneously and in which his attention is directed according to need, both to the receiving of the stimulus and the making of the reaction. Considerable practice is necessary to develop the extreme forms, and there is at first a tendency to revert to the 'natural' or intermediate type. The short extreme is developed by slurring the sensory part more and more; the long, on the contrary, by directing attention to it. In both

of these modes of reaction in which the attention is directed by all uniformly to a certain part of the process, the differences between the subjects tend to disappear. In the intermediate form, however, these differences remain prominent, though to a lessening degree with practice; and they are attributed to constant differences in the direction of the attention.

The author's mode of tabulation, and especially his transit experiments with the 'natural' mode of reaction, enable him to give an excellent account of the production of these longer and shorter forms of reaction. But while it is rather easy to see how the subjects may pass by training from the 'natural' mode of reaction, on the one hand, to the quicker and more nearly automatic; and, on the other, to the more cautious and hesitating type, in which a somewhat distinct interval is recognized between the perception of the stimulus and the making of the reacting movement, and how various conditions might lead the reactor to pass from one to the other, or to stop at different stages, some of which would naturally come to be preferred by practice—several peaks are found in the frequency curves of the long reactions—it may be questioned whether the author is right in assuming, with others of the Wundtian school, that turning the attention to the reception of the stimulus will always result in the longer forms of reaction, or that sensory reactions and these longer forms are one and the same thing. They may very well be separable, and the direction of the attention to the stimulus may not be the essential thing in the case. The evidence offered by American observers in which the sensory reactions are as short or shorter than the motor, would give this hypothesis a certain degree of probability. Even that the longer reactions are to a proportional extent more complete, may be doubted and should not be assumed without analysis.

The chief causes of the large individual differences found in transit observations are lack of practice, the use of different modes of reaction, and the following of the star with the eye instead of fixating the cross-lines. The shorter and so the more uniform mode of reaction is the better for astronomical work, though an extremely short form cannot be attained, because the stimulus demands some attention. The 'natural' mode of reaction, however, permits the analysis of the psychological conditions the better and so was generally employed. The long form cannot be used as the moving star almost unavoidably prepares the mind for the shorter reactions. The movement of the star must, however, be of a certain rate for this to be true, namely, a movement between 10° and 14° per second. Lower speed occasions long re-

actions and quicker marked irregularities—the extent of the field of view remaining the same, namely, about 28°. As the entrance of the star into the indirect field of vision served as a warning signal, this makes the favorable preparation interval from 1 to 1.5 seconds. To a certain extent, the smaller the field of view with a rate of 14° per second, the quicker and more uniform the reactions even when the preparation time was reduced considerably below a second. The direction of motion of the star makes little difference if the cross-lines are fixated in the observations, but it is otherwise when the eye follows the star since a movement from left to right as compared with a movement from right to left causes a lengthening of the records, if the right eye is employed.

Astronomers use eyepieces with several cross-lines, and other stars beside the one observed may be seen moving across the field of view. When these conditions were reproduced artificially, it was found that the record became shorter and more uniform as a result of the intensifying effect of the rhythm of observation and movement developed.

As in this instance, the successful application of psychology to a practical problem may have to wait long for fundamental discoveries to be made, but when such application is made it certainly deserves a special welcome.

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PATHOLOGICAL.

De l'action de la fatigue sur la structure des cellules nerveuses de l'écorce. G. GUERRINI. Archives italiennes de Biologie, XXXII., Fasc. I. Pp. 62–64.

After a brief review of the results in this direction obtained by Tissié, Hodge, etc., the present investigator in this 'preliminary note' describes his own work, emphasizing especially that therein for the first time the stimuli to fatigue were purely physiological in their nature, and that their duration and the intensity of the resulting fatigue were both exactly measured. Twelve dogs were induced to run distances varying between 35 km. and 98 km., until obviously fatigued. The animals were then killed, and, under special precautions, their cortical cells, both cerebral and cerebellar, were examined. The alterations found were 'both numerous and interesting,' and may be summarized as follows:

"1. Augmentation of the pericellular lymphatic spaces. 2. The presence of leucocytes on the cell in the pericellular space. 3. Disorder of

the achromatic network. 4. Decomposition of the chromatic plaques, which were sometimes reduced to mere bits. 5. The presence of vacuoles in the protoplasm. 6. An irregular appearance of the contour of the nucleus. 7. The presence of vacuoles in the nucleus." These the researcher considers to be proportional to the fatigue displayed by the animal rather than to the distance he had run. They are more evident in the cerebral cortex than in the cerebellar cortex, and are always more distinct about the 'motor zones.'

Sur le mode de so comporter des différentes sensibilités sous l'action de divers agents. DRS. G. BOERI and R. DI SILVESTRO, Naples. Archives italiennnes de Biologie, Vol. XXXI. Pp. 460–464.

This article is its authors' summary of the results they obtained from a study of the different dermal sensations and of the muscular sense under various pathological conditions artificially produced for the purpose, with especial reference to their respective degrees of persistency. As subjects they employed themselves chiefly, but also some women far advanced in pregnancy and others with cirrhotic, much enlarged livers. For instruments they employed, in studying touch, Weber's compass; pressure, Eulemburg's baresthesiometer; pain, the faradic apparatus of Lewandowski; in studying thermal sensibility, a thermesthesiometer invented by one of themselves; and for the muscular-sense investigation, they used a wire skeleton covered with cloth on which weights were placed. In each case they first studied the individual in the normal condition.

The effect of *distention* of the skin was studied on the abdomens of the women subjects. *Compression* of a nerve was produced, in the case of the ulna, by digital compression, and in the sciatic by resting a long time on a horizontal bar. *Refrigeration* was produced by placing the member to be used in melting snow, with or without salt. Artificial *anæmia* was brought about by application of the expulsive bandage of Esmarch. *Hyperæmia* was somewhat similarly produced by mere constriction until the limb was purple.

Their results may perhaps be best given in their own terms: "We found sensibility to *pain* to be the most resistant of all to the agents to which they were submitted, save that to cold it was the least resistant of all. *Thermic* sensibility is, next to pain, the most resistant, with the same exception as regards refrigeration, where the contrary is the case (thermic sensibility is more resistant to warmth, or, stated better, by our mode of research, augmentations of heat are better determined than lowerings of temperature). Sensibility to *pressure* acts

somewhat as does the tactile sense in that one cannot always say which of the two is the least persistent; both, and perhaps especially sensibility to pressure are little resistant, except to refrigeration, to which the latter is very resistant. The *muscular* sense also preserves itself well in cold, diminishes a little with compression, and disappears with anæmia produced à la Esmarch."

These sensibilities have, the authors think, discriminating physiological differences which make them properly considered as independent of each other, for in unnatural conditions like these they vary differently, one or the other diminishing more than the others.

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NEW BOOKS.

La Philosophie d'Auguste Comte. L. LÉVY-BRUHL. Paris, Alcan. 1900. Pp. 417.

Wirklichkeiten; Beiträge zum Weltverständniss. K. LASSWITZ. Berlin, E. Felber. 1900. Pp. vi + 444.

Monistische Gottes und Weltanschauungen. J. SACK. Leipzig, Engelmann. 1900. Pp. viii + 278.

NOTES.

MR. WILLIAM RICHARD SORLEY, professor of moral philosophy in the University of Aberdeen, has been elected to the Knightbridge professorship of moral philosophy at Cambridge University, in the place of Professor Henry Sidgwick who has been compelled to resign owing to ill health.

Professor Th. Ziehen, associate professor of psychiatry in the University at Jena, has been called to the University of Utrecht.

W. D. SCOTT, Ph.D. (Leipzig), has been appointed to the newly established instructorship in psychology and pedagogy in the Northwestern University.

DR. GEORGE V. N. DEARBORN has been appointed assistant professor of physiology in the Tufts College Medical School.

DR. S. E. MEZES of the University of Texas has been promoted from an associate to a full professorship of philosophy.

PROFESSOR LUCIANI, rector of the University of Rome, has celebrated the completion of his 25th year of teaching. Exercises were held in the physiological laboratory of the University, and addresses were made by Professors Todaro and Baccelli to which Professor Luciani replied.

ANOTHER eminent Italian neurologist, Professor Camillo Golgi, has been made a senator of the Kingdom.

PROFESSOR J. MARK BALDWIN, of Princeton University, has returned to the United States after a residence of over a year at Oxford, where he had been seeing through the press the 'Dictionary of Psychology and Philosophy' shortly to be published by The Macmillan Company.

PLANS have been proposed for the establishment in Paris of the 'International Society of the Psychical Institute,' the aims of which, according to a circular signed by M. Pierre Janet, are:

1. To collect in a library and museum all books, works, publications, apparatus, etc., relating to psychical science.

2. To place at the disposal of researchers, either as gifts or as loans, according to circumstances, such books and instruments necessary for their studies as the Institute may be able to acquire.

3. To supply assistance to any laboratory or to any investigators, working singly or unitedly, who can show that they require that assistance for a publication or for a research of recognized interest. This function, which has been fulfilled so usefully by the Société pour l'Avancement des Sciences, in relation to the physical sciences, must also be discharged by the new Institute in relation to mental science.

4. To encourage study and research with regard to such phenomena as may be considered of sufficient importance.

5. To organize lectures and courses of instruction upon the different branches of psychical science.

6. To organize, as far as means will allow, permanent laboratories and a clinic, where such researches as may be considered desirable will be pursued by certain of the members.

7. To publish the 'Annales de l'Institut Psychique International de Paris,' which will comprise a summary of the work in which members of the Institute have taken part, and which may be of a character to contribute to the progress of the science.

Founders, donors, and ordinary members are wanted for the Institute, the first class are to give 10,000 fr. or more, the second 1,000 fr. or more, while ordinary members pay an annual subscription of 20 fr. An international council of organization has been formed in which America is represented by Professor James. Professor J. Mark Baldwin, Princeton, N. J., is a member of the committee of patrons, and will send circulars to those making application for them.